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Problems in the Historical Study of Power in the Cities and Towns of the United States, 1800–1960

DAVID C. HAMMACK

POWER, LIKE WEALTH AND SOCIAL STATUS, seems always to be distributed unequally, but the nature, the degree, the causes, and the consequences of inequality have surely varied from time to time and place to place. Although historians of American communities since 1800 have often been concerned with the distribution of power, they have usually approached the matter indirectly, implicitly, and incompletely. Much of the best recent work on nineteenth-century communities has neglected power in order to emphasize other dimensions of social life and has sought to shift attention from the elite to the “inarticulate.” But power is as important to community life as wealth and status are; indeed, what we always want to know about wealth and status is their relationship to power. The powerless may face difficulties fully as unpleasant as those of the poor or the outcast. Yet, as many recent works have insisted, these three disadvantages need not always afflict the same people: the most impoverished workers, the most despised immigrants, and even slaves have, it is said, exerted sufficient power to shape the worlds they inhabited and to justify a sense of collective pride.

Although power is exercised in specific places at specific times, the general history of power in the towns and cities of the United States is a story worth working out, whether our larger purpose is the evaluation of American democracy, the dispassionate analysis of social structure, or the comparison of American communities with those of other nations. Taken together, the older and newer historians of American communities, often drawing on the classics of Western social and political theory, have advanced or supported six conflicting generalizations about the course of power since the eighteenth cen-

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ture. Three of these generalizations stress the continuing dominance of elites, asserting variously that older and established upper classes have successfully retained power by absorbing newcomers thrown up—as at the end of the nineteenth century—by new forms of capitalism, that the new capitalists displaced the old elites as in the 1830s or 1930s, or that a unified, exclusive, self-perpetuating upper class has dominated throughout. The other three interpretations stress elements of diversity in the distribution of power; they emphasize the continuing, declining, or increasing diversity of power-wielders over time. These six generalizations have, in turn, been supported by a wide variety of interpretations of the relationship between power and society. Explicitly or implicitly, historians have advanced all the classic interpretations of power, relating it in various ways to wealth, economic organization, social stratification, political institutions, and ideology and suggesting that these relationships were further shaped by such factors as the population size, age, region, and economic history of the community.

American historians' interpretations of power conflict at many points and cannot easily be reconciled; they are quite complex and involve, for many scholars, fundamental political beliefs and preferences. Even framing generally acceptable definitions for such terms as "power" and "community" is difficult, and devising generally accepted methods for their study is almost impossible. The English philosopher Steven Lukes has insisted that power is "an essentially contested concept," for which any definition—and any research strategy—is "inextricably tied to a given set of (probably unacknowledged) value-assumptions."¹ Lukes puts the point very strongly, but there is no doubt that it is difficult to study power, that there is no unambiguous and generally accepted way to produce a series of Gini coefficients for the distribution of power similar to those Lee Soltow has derived for the distribution of wealth.² Despite the difficulties, it is important to seek common ground; the history of power is at least as interesting as the history of wealth, and a general history of community power in the United States will require a number of studies by different investigators. At present, none of the leading propositions about the history of power can be evaluated against a set of studies which clearly, consistently, and comparably describe the distribution of power in any group of communities since the eighteenth century, let alone in a representative group over the entire period. Nevertheless, a large number of disparate works do touch on the issue, at least indirectly; future studies can and should build on a substantial body of existing knowledge and hypotheses. If we cannot at present produce a history of community power in the United States, we can at least identify the chief areas of conflict in existing interpretations, the gaps in our knowledge, and some plausible hypotheses. Knowing the problems, we can select the methods most appropriate for their solution.

¹ Lukes, *Power: A Radical View* (London, 1974), 26.

² Soltow, *Men and Wealth in the United States, 1850-1970* (New Haven, 1975). There is some disagreement among economists about the best way to measure inequalities of income and wealth; see, for example, Morton Paglin, "The Measurement and Trend of Inequality: A Basic Revision," *American Economic Review*, 65 (1975): 598-609.

The voluminous social science literature on community power can also contribute to a history of the subject. Sociologists and political scientists disagree, at least as much as do historians, about the study of power, but more than most historians these social scientists have sought to make their definitions, methods, and conclusions clear and explicit. They have, moreover, studied the distribution of power in a variety of communities from the 1920s to the present; a few have even ventured bravely into historical research. Historians can gain a good deal from these debates over definitions, problem-formulation, and methods and can use the conclusions to add a twentieth-century dimension to the history of community power. But the social science literature cannot be used casually; it is full of subtle and complex controversy. Effective historical use of the social science work on community power must recognize these controversies and take a position on them. At the same time, the sociological study of community power stands very much in need of deeper historical and comparative perspectives.

WITH SOME EXCEPTIONS, most empirically oriented social scientists have accepted one or another variant of Max Weber's definition of power as "the chance of a man or of a number of men to realize their own will in a communal action even against the resistance of others who are participating in the action."³ Under this definition, power is a relative term, and one person may be said to exert more power than another when he or she attains his or her objective, whether opposition develops or not, whether the result is achieved publicly or behind the scenes, and whether or not others regard the result as legitimate. Power may be exercised by individuals or by collectivities, from families to voluntary associations to social classes. Social scientists have also generally accepted the distinctions (implied by Weber's definition and stated in these terms by Robert A. Dahl) between the "scope," "strength," and "extension" of a person's power. "Scope" refers to the number of areas of community life dominated by the powerful; "strength," to the relative ability of the powerful to overcome those who resist; and "extension," to the number of people the powerful person or group dominates.⁴ There is some disagreement as to the best interpretation of "communal" in Weber's definition—is the arena within which the distribution of power is to be assessed to be the unit of local government, or is it to be some combination of local and regional governments? Is community power properly limited to the ability to

³ Weber, "Class, Status, Party," in *From Max Weber: Essays in Sociology*, ed. and trans. H. H. Gerth and C. Wright Mills (New York, 1946), 180. In a comprehensive and acute recent contribution to the definition of power, Jack H. Nagel has reformulated Weber's definition: "a power relation, actual or potential, is a causal relation between the preferences of an actor regarding an outcome and the outcome itself"; Nagel has shown, moreover, that his definition incorporates—or is consistent with—other proposed definitions, some of which can best be considered to be empirical hypotheses; see *The Descriptive Analysis of Power* (New Haven, 1975), 29–31, 91–100, 175–77. For two excellent collections of articles on power, see Roderick Bell, David V. Edwards, and R. Harrison Wagner, eds., *Political Power: A Reader in Theory and Research* (New York, 1969); and Willis D. Hawley and Frederick M. Wirt, eds., *The Search for Community Power* (Engelwood Cliffs, N.J., 1968).

⁴ Dahl, "A Critique of the Ruling Elite Model," *American Political Science Review*, 52 (1958): 463–69, and "The Concept of Power," *Behavioral Science*, 2 (1957): 201–15.

influence units of government or does it include the ability to influence voluntary associations, business enterprises, and other institutions as well? The greatest controversies in the social science literature on community power concern theories, findings, and methods. It is best to leave the methodological debate aside until we are ready to consider methods appropriate to historical research, but we cannot ignore the debates over theories and findings.

The leading controversy about community power theory has to do with the relation between power and the social order and, ultimately, with the impact of economic development and the increasing scale of most urban communities on the distribution of power. In this controversy the four main positions hold that the distribution of power always follows the distribution of wealth (the stratificationist position); that power is the product of vigorous organization, whether the basis of organization is economic, ethnic, religious, regional, or whatever (a pluralist position); that the distribution of power probably varies according to several economic, demographic, organizational, institutional, and ideological factors (a preliminary multivariate approach to a theory); and that not enough is known to construct a theory of community power because too few rigorous, comparable studies of power distributions have been completed (the skeptical position).⁵ Although the controversy over theory generates much of the energy that sustains community power research, it is not necessary to embrace one of the theoretical positions in order to use the social science literature. As Nelson Polsby has observed, the controversies over findings, methods, and theories can be sharply distinguished from one another; advocates of each theoretical position have used every method, and many investigators have found the distribution of power in a particular community to be different from the distribution which theory had led them to expect.⁶

For the historian, knowledge of the social scientists' conflicting findings about the distribution of power in mid-twentieth-century communities can help clarify the historical and theoretical issues and provide a necessary orientation for the debate over methods. Initially, sociologists and political scientists put forward two opposing descriptions of the distribution of power, based largely on studies conducted in several American communities during the 1950s and 1960s. One description supported the stratificationist, the other, the pluralist position. As Polsby demonstrated in his brilliant critique of the literature to 1962, many sociologists followed Robert and Helen Lynd and

⁵ For leading statements of the stratificationist position, see Peter Bachrach and Morton Baratz, *Power and Poverty: Theory and Practice* (New York, 1970); and Matthew Crenson, *The Un-Politics of Air Pollution: A Study of Non-Decisionmaking in the Cities* (Baltimore, 1971). On the pluralist position, see Robert A. Dahl, *Who Governs? Democracy and Power in an American City* (New Haven, 1961). On the preliminary multivariate position, see Michael Aiken, "The Distribution of Community Power: Structural Bases and Social Consequences," in Michael Aiken and Paul E. Mott, eds., *The Structure of Community Power* (New York, 1970), 487-525. On the skeptical position, see Nelson W. Polsby, "'Pluralism' in the Study of Community Power: or, *Erklärung* before *Verklärung* in *Wissenssociologie*," *American Sociologist*, 9 (1969): 118-22. As Polsby points out, the term "pluralist" has lost much of its meaning through imprecise use; it is employed in this article only so far as necessary to describe positions in the debate through the 1960s.

⁶ Polsby, "'Pluralism' in the Study of Community Power."

Floyd Hunter in arguing that the following set of propositions characterized the cities and towns they had studied:

1. There is an upper class power elite which rules in local community life.
2. Political and civic leaders are subordinate to this elite.
3. There is only one elite in each community, possessing a common will, and exerting dominance on substantially all nontrivial community issues.
4. The elite rules in its own interests exclusively, and largely to the detriment of the lower classes.
5. As a consequence, social conflict takes place between the elite and the nonelite.⁷

Polsby and others have advanced telling arguments against much of the research on which the sociologists based these conclusions and have argued that sounder studies of cities as diverse as New Haven, New York, Syracuse, and Chicago during the 1950s demonstrated a pluralistic distribution of power, in which "participation in decision-making is limited to a relatively few members of the community, but only within the constraints of a bargaining process among competing elites and of an underlying consensus supplied by a much larger percentage of the local population, whose approval may be difficult to secure."⁸ These studies agree that each elite participates only in decisions concerning a limited scope of issues and that, although successful participation in decision making requires possession of some resources, power in this sense is less strongly related to wealth, economic position, or social prestige than to the participant's persistence and skill.

These "pluralist" findings have in turn been criticized by Peter Bachrach, Morton Baratz, Matthew Crenson, Steven Lukes, and others, who have advanced new reasons for concluding that a single upper class power elite dominates in at least some communities.⁹ Pointing out that the pluralist conclusion acknowledges the dominant role of elites, those who support neostratificationist conclusions suggest that there is not much difference between rule by a single dominant elite and by a few elites; as Crenson has put it, "the pluralism of observable political reality may actually be a rather stunted kind of diversity, hedged about by concentrations of political influence which prevent the further growth of local political heterogeneity."¹⁰ These writers argue further that the dominant elites use behind-the-scenes methods to control the decision-making process and manipulate the "underlying consensus" so as to organize some issues out of politics, suppress proposals inimical to their interests, and even persuade people to act against

⁷ Nelson W. Polsby, *Community Power and Political Theory* (New Haven, 1963), 7-11.

⁸ Wallace S. Sayre and Nelson W. Polsby, "American Political Science and the Study of Urbanization," in Philip Hauser and Leo Schnore, eds., *The Study of Urbanization* (New York, 1965), 132.

⁹ Bachrach and Baratz, *Power and Poverty: Theory and Practice*; Crenson, *The Un-Politics of Air Pollution*; and Lukes, *Power: A Radical View*.

¹⁰ Crenson, *The Un-Politics of Air Pollution*, 25.

their own self-interest.¹¹ Pluralists respond that these “hidden faces” of power are difficult to measure and that, in any case, elites also respond to a barely visible form of power when they shape their behavior in accordance with the anticipated reactions of their constituents or publics.

There is a timeless, as well as a timebound, quality to most of the social science literature on community power, although a few social scientists have looked into the history of the communities they have studied.¹² But in recent years a number of sociologists have moved toward a historical perspective by undertaking comparative analyses of the entire community power literature. Setting methodological strictures aside, these sociologists take the findings of each study at face value and range them along a continuum from a concentrated to a dispersed distribution of power. They then use multivariate or multiple regression statistical techniques to seek the characteristics associated with communities at each end of the continuum. Their results variously suggest that a dispersal of power is associated with relatively larger size, greater social heterogeneity, more economic diversity, increasing absentee ownership of local economic enterprises, a larger number of voluntary associations, and a higher level of citizen participation in local politics.¹³ Michael Aiken concludes that “the older cities, those located in regions that were industrialized earlier, those having had a greater influx of immigrants and other minority groups, and those having non-reformed formal political structures, are cities that are most likely to have decentralized decision-making arrangements.”¹⁴ These suggestions embody implicit theses about the history of power and are in contrast to the stratificationists’ apparent view that a small, unified elite has held power always and everywhere. Derived from studies of varying quality, which employed diverse research methods and rarely paid serious attention to history, these conclusions are plausible but by no means firmly established. They do suggest a set of problems for future historical research, as other evidence will indicate.

HISTORICAL WORKS ON COMMUNITY POWER support six historical versions of the stratificationist and pluralist interpretations of power. Many historians have always accepted the proposition that wealth is the basis of power and, finding

¹¹ For the most comprehensive review and statement of these arguments, see Lukes, *Power: A Radical View*, 16–35.

¹² For some notable examples, see the work of Robert Schulze, Wallace Sayre and Herbert Kaufman, Donald Clelland and William Form, Donald Bradley and Mayer Zald, and Peter and Alice Rossi. For a review of this literature, see notes 67 and 69, below.

¹³ David Rogers, “Community Political Systems: A Framework and Hypothesis for Comparative Studies,” in Bert E. Swanson, ed., *Current Trends in Comparative Community Studies* (Kansas City, Mo., 1962), 31–48; Terry N. Clark, “Power and Community Structure: Who Governs, Where, and When?” in Charles Bonjean *et al.*, eds., *Community Politics: A Behavioral Approach* (New York, 1971), 174–87; Aiken, “The Distribution of Community Power”; John Walton, “The Vertical Axis of Community Organization and the Structure of Power,” in Bonjean *et al.*, *Community Politics*, 188–97, and “A Systematic Survey of Community Power Research,” in Aiken and Mott, *The Structure of Community Power*, 443–64; and Claire W. Gilbert, “Some Trends in Community Politics: A Secondary Analysis of Power Structure Data from 166 Communities,” in Bonjean *et al.*, *Community Politics*, 210–15.

¹⁴ Aiken, “The Distribution of Community Power,” 506.

wealth distributed unequally throughout American history,¹⁵ have stressed the continuing dominance of small, well-to-do elites. For many years, most historians of American cities implicitly adopted, at least for the period 1830–1910, a patrician elitist theory of power which received its fullest and most influential exposition in the work of James Bryce. In *The American Commonwealth* (1888), Bryce claimed that the experience of Western Europe since the Roman Republic showed that “when, in a large country . . . the sphere of government widens, when administration is more complex and more closely interlaced with the industrial interests of the community and of the world at large . . . , the business of a nation falls into the hands of men eminent by rank, wealth and ability, who form a sort of governing class, largely hereditary.”¹⁶ Although he quickly noted that the “best men,” as he called the members of the “governing class,” usually did not hold office in American cities, Bryce found a way to reconcile American facts with his European expectations. Following the views of leading university administrators, lawyers, journalists, and upper-class reformers in a dozen cities, Bryce held that

1. There are two powerful elites in American cities, the new industrial capitalists and the established economic, social, and cultural elite Bryce called the “best men.”
2. These elites are the true rulers of their communities; office holders and political party leaders are subordinate to them.
3. Both the industrialists, through their wealth, and the best men, through their domination of the law and of public opinion, exert influence on every issue they deem important; the industrialists, however, are concerned only with a narrow range of issues which affect their own interests, while the best men have too often failed to take part in political life because they understood neither their duty nor their highest self-interest.
4. Although the industrialists seek to rule in their own best interests, the best men pursue the general interest of the community as a whole.
5. Significant conflict occurs chiefly between the best men and the industrialists; the bosses have no independent base of power and no concern with substantive policy; the poor, the immigrants, and the blacks are ciphers. If the best men would only exert themselves, they would gain the support of “well-conducted men of small means” and of the politically active poor and could use their latent power to govern the cities of the United States.¹⁷

¹⁵ For a recent discussion of the evidence on the history of wealth distributions, see Stanley Lebergott, “Are the Rich Getting Richer? Trends in U.S. Wealth Concentration,” *Journal of Economic History*, 36 (1976): 147–62. Soltow also points to the remarkable consistency in the share of wealth held by the wealthiest 1 percent of the population between 1850 and 1950; *Men and Wealth in the United States*, chap. 5. Also see Gabriel Kolko, *Wealth and Power in America: An Analysis of Social Class and Income Distribution* (New York, 1962).

¹⁶ Bryce, *The American Commonwealth*, 2 (New York, 1888): 26.

¹⁷ For this reading of *The American Commonwealth*, see my “Participation in Major Decisions in New York City, 1890–1900: The Creation of Greater New York and the Centralization of the Public School System”

Bryce's interpretation of power differs from stratification theory primarily in dividing the wealthy into two groups and attributing greater virtue to one of them. Like the theories of recent social scientists, Bryce's interpretation had an attenuated historical dimension. He associated increasing economic and political complexity with the rise of elite domination. Acknowledging that a new elite of industrial capitalists had risen since the 1850s, he thought it probable that the old elite—"men eminent by birth, wealth, and ability"—would retain its dominance and, through the universities and other institutions of genteel culture, absorb the new industrialists and their children.

A few Americans had urged elements of the patrician elitist theory on Alexis de Tocqueville in the 1830s, but Bryce found support for it almost universal among those in a dozen cities with whom he conducted a voluminous correspondence from the mid-1870s through World War I.¹⁸ Perhaps because they found it accepted in polite society, in *The American Commonwealth*, and in the papers of leading municipal statesmen, the first generation of American urban historians implicitly accepted Bryce's version of the patrician elitist theory of power. Although Arthur M. Schlesinger largely neglected the study of power in his later work, characteristically devoting only nine pages to the subject in *The Rise of the City*, his notes for those pages referred exclusively to Bryce and Bryce's American informants.¹⁹ Schlesinger shared the patrician elitist view that the "better classes" could and should govern the cities, and he observed that "English travellers were naturally much struck by the divorce between politics and . . . the higher culture."²⁰ Schlesinger also agreed that the avarice of big business, the venality of the bosses, and the indifference of the "better classes" produced bad government; and he ignored, like Bryce, the role of immigrant groups in city politics.²¹ Many of the authors of "city biographies" who studied with Schlesinger implicitly used the patrician elitist view of power to organize their works. Less inclined than Bryce to write in terms of class, they made his emphasis on the economic and social standing of the best men implicit and stressed their superior moral and intellectual qualities. In Bessie Louise Pierce's account of Chicago, for example, the Citizens' Association (composed, as she indicated in scattered passages, of Chicago's great entrepreneurs) ruled the city in the general interest, interrupted only by the colorful appeals of wealthy political adventurers to an ignorant electorate.²² Blake McKelvey and Clifford Patton have also written in the patrician elitist

(Ph.D. dissertation, Columbia University, 1973), chap. 3, and my "James Bryce and the Perception of Power in the Cities of the United States, 1880-1900," in H. C. Allen and C. W. E. Bigsby, eds., *Anglo-American Cross-Currents* (forthcoming).

¹⁸ Hammack, "James Bryce and the Perception of Power"; and Stow Persons, *The Decline of American Gentility* (New York, 1973), 24-26.

¹⁹ Schlesinger, *The Rise of the City, 1878-1898* (New York, 1933), 387-96.

²⁰ *Ibid.*, 388.

²¹ *Ibid.*, 387, 389-92.

²² Bessie Louise Pierce, *A History of Chicago*, vol. 3: *The Rise of a Modern City* (New York, 1957), 9-15, 270, 301-08, 321-80. Though not a student of Schlesinger, Constance M. Green adopts a similar view of the distribution of power; see her *Holyoke* (New Haven, 1939), 205-06, 225, 270-72, 297-301, chap. 13, and her *Washington, Capital City, 1879-1950* (Princeton, 1963), 4-5, 15-25, 30-33, 35-58, 61-76. In both *Holyoke* and *Washington* Green describes a social and economic elite which, when it chooses to act, easily dominates immigrants, blacks, and corrupt businessmen and politicians.

tradition, modifying it only by their belief, shared with Lincoln Steffens, that sheer enthusiasm and goodness endowed some of the best men with a non-material source of power.²³ Allan Nevins and his associates similarly relied implicitly on the patrician elitist theory of power in their biographies of such civic leaders as Abram S. Hewitt, Grover Cleveland, Samuel J. Tilden, and William C. Whitney. These works provide a great deal of information on political disputes and business arrangements among the wealthy.²⁴ But in each of them the master conflict is between the rich, well educated, and good, on one side, and the corrupt and ignorant, on the other. Taking the patrician elitist theory of power for granted, these historians neither tested nor refined it. But the theory served them well; extensive research apparently gave them no reason to rethink their organizing assumptions.²⁵

A second group of historians, relying more on the writings of intellectuals and literary men than on the views of activist reformers or the details of political or social welfare history, has advanced a thesis of genteel decline, arguing that previously powerful elites of gentlemen lost out to mere wealth some time between 1830 and 1910.²⁶ Richard Hofstadter's chapter on "The Status Revolution and Progressive Leader" in *The Age of Reform* is still the most prominent statement of this view. Underlying his disputed assertions about the "socio-psychological tensions" afflicting progressive and pre-progressive leaders, Hofstadter posited the existence in many cities and towns of a class of well-to-do, cultivated business and professional men who "suffered from the events of their time not so much through a shrinkage in their means but through the changed pattern in the distribution of wealth and power."²⁷ Frederic C. Jaher's essays on the social elites of Boston and New York City also place the decline in their power in the decades following the Civil War, while Stow Persons, who relies on similar evidence from socially elite writers, argues that the "decline of American gentility" began as early as the 1850s and was not complete until after the First World War.²⁸ A number of

²³ Blake McKelvey, *The Urbanization of America, 1860-1915* (New Brunswick, N.J., 1963), 107, chaps. 6, 7; and Clifford Patton, *The Battle for Municipal Reform* (Washington, 1940), 3-4. Pierce, McKelvey, and Patton all worked with Schlesinger.

²⁴ Allan Nevins, *Grover Cleveland: A Study of Courage* (New York, 1932), 34-45, 65-70, 443-52, and *Abram S. Hewitt, with Some Account of Peter Cooper* (New York, 1935), chaps. 6-7, 9, 11-14, 22; Mark D. Hirsch, *William C. Whitney, Modern Warwick* (New York, 1948), 179, 192-226, 421-68; and Alexander C. Flick, *Samuel Jones Tilden: A Study in Political Sagacity* (New York, 1939), 115-17, 161-65, 239-41. Wyatt W. Belcher, another student of Nevins, adopts a similar view of power; see *The Economic Rivalry between St. Louis and Chicago, 1850-1880*, Columbia University Studies in the Faculty of Political Science, no. 529 (New York, 1947), esp. chaps. 1, 7.

²⁵ The critics of Schlesinger, Nevins, and their students have not recognized the implicit theory of patrician elitism in their works. See, for example, Thomas J. Condon, "Politics, Reform, and the New York City Election of 1886," *New York Historical Society Quarterly*, 44 (1960): 375; and Samuel P. Hays, "The Social Analysis of American Political History, 1889-1920," *Political Science Quarterly*, 80 (1965): 375, and "The Changing Political Structure of the City in Industrial America," *Journal of Urban History*, 1 (1974): 6-7.

²⁶ For two political studies that reach this conclusion, see Dixon Ryan Fox, *The Decline of Aristocracy in the Politics of New York, 1801-1840* (New York, 1919); and Carl Russell Fish, *The Rise of the Common Man, 1830-1850* (New York, 1927).

²⁷ Hofstadter, *The Age of Reform: From Bryan to FDR* (New York, 1955), 135. For an explicit discussion of Hofstadter's sociopsychological theory of power, see *ibid.*, 153.

²⁸ Jaher, "The Boston Brahmins in the Age of Industrial Capitalism," in Jaher, ed., *The Age of Industrialism in America: Essays in Social Structure and Cultural Values* (New York, 1968), 188-262, "Nineteenth-

other studies have argued that a shift of this sort took place in particular communities during the 1800s, the 1830s, and the 1920s.²⁹ The thesis of genteel decline insists that the growing size of the cities, and the increasing complexity of their economic and political institutions, had an effect quite different from that seen by Bryce and his American informants and followers.

Rejecting the the patrician distinction between the “best men” and the newly rich, a third group of historians has adopted a bald version of the stratificationist theory, holding that the distribution of wealth determines the distribution of power and that unified, self-perpetuating if not always hereditary upper-class elites have dominated American cities at least from the early decades of the nineteenth century. According to this view, what has changed is simply the means by which the upper class exercises its power. Reviewing the literature of officeholding in antebellum cities, Edward Pessen claims that “antebellum urban governments, like influential voluntary associations, urban society, and the urban economy appear to have been dominated by small upper classes.”³⁰ Michael B. Katz concurs, arguing “that the entrepreneurial class comprised an overlapping elite governing economic, political and associational life” in mid-nineteenth-century Hamilton, Canada. Distinguishing between enduring social patterns and individual life histories, he adds that “individual members of this class, however, did not remain secure in their exercise of power and privilege.” In a study which goes well beneath the external evidence of officeholding, Michael H. Frisch suggests that as late as the 1870s in Springfield, Massachusetts upper-class domination was accepted throughout all ranks of society as a given not to be questioned.³¹

The historical stratificationists note that the upper class did stop holding office sometime after the middle of the century but argue that it continued to dominate through a pattern first identified by Moisei Ostrogorski in his *Democracy and the Organization of Political Parties* (1902), a work which historians of cities have generally ignored. In an implicit critique of contemporary patrician elitists, Ostrogorski denied that the “best men” could be distinguished from the industrial capitalists. Observing that municipal reform candidates “were not successful in bridging the gulf between the people and the rich,” he asserted that reform clubs “were only ‘bourgeois’ associations” which often “aimed deliberately” to exclude the poor and sought “to bind together the ‘classes,’ not exactly against the ‘masses’ as such, but against the masses who

Century Elites in Boston and New York,” *Journal of Social History*, 6 (1972): 32–77, and “Style and Status: High Society in Late Nineteenth-Century New York,” in Jaher, ed., *The Rich, the Well Born, and the Powerful: Elites and Upper Classes in History* (Urbana, Ill., 1973), 258–84; and Persons, *The Decline of American Gentility*, 131–78.

²⁹ Benjamin W. Labaree, *Patriots & Partisans: The Merchants of Newburyport, 1764–1815* (Cambridge, Mass., 1962), chap. 5; Robert G. Albion, *The Rise of New York Port, 1815–1860* (New York, 1939), chap. 12; Fox, *The Decline of Aristocracy*, chap. 14; and Adolf A. Berle and Gardiner C. Means *The Modern Corporation and Private Property* (New York, 1934), 119–25.

³⁰ Pessen, “The Social Configuration of the Ante-Bellum City: An Historical and Theoretical Inquiry,” *Journal of Urban History*, 2 (1976): 296.

³¹ Katz, *The People of Hamilton, Canada West: Family and Class in a Mid-Nineteenth-Century City* (Cambridge, Mass., 1975), 177; and Frisch, *Town into City: Springfield, Massachusetts, and the Meaning of Community, 1840–1880* (Cambridge, Mass., 1972), 242.

'by supporting plundering politicians . . . allowed serious injury to be inflicted on the propertied classes.'''³² Applying his realistic approach to the politicians and voters as well as to the wealthy, Ostrogorski offered an account of power which can be summarized in four points:

1. A capitalist elite rules America's cities. The established social, economic, and professional elites are to all purposes (if not intents) effectively a part of the capitalist elite.
2. Political party leaders and officeholders are subordinate to this elite, but they cannot easily be displaced.
3. The capitalist elite exerts dominant influence on every issue important to its economic interest and usually ignores the broader public interest.
4. The political leaders mediate between the needs of the capitalists and the mass of voters; the voters are not ignorant of their own interests; potential conflict lies between the propertied class and the propertyless; when the politicians fail to prevent this conflict from breaking out, or ally themselves too closely with the propertyless, the propertied successfully rally to protect their interests.³³

Like Ostrogorski, historical stratificationists from Gabriel Almond to James Weinstein and Gabriel Kolko have stressed the importance of indirect control over the parties and the machinery of local government in the twentieth century.³⁴

ALTHOUGH MOST HISTORIANS WHO HAVE FOCUSED ON ELITES have stressed the concentration of power, those who have written detailed studies of entrepreneurship, social policy, lower status groups, or local politics in the last twenty-five years have more frequently insisted that power was dispersed. Some of the most striking works of this sort find a significant increase or decrease in the dispersion of power; but most works, covering only a brief span of time, suggest that no single elite or pair of elites dominated during the years under consideration.

Historians who have found power to be widely dispersed, detecting no long-term shift—our fourth group—have focused on several different research problems. Those concerned with the relationship between the entrepreneur and the community have been at pains to deny that power was ever concentrated in the hands of one or two wealthy elites. Entrepreneurs, they insist, have never been sufficiently powerful to act as simple "economic men"; they

³² Ostrogorski, *Democracy and the Organization of Political Parties*, 2 (New York, 1902): 248-49, 92-94, 191-95, 246-48, 289-90.

³³ For this interpretation of Ostrogorski's *Democracy and the Organization of Political Parties*, see Hammack, "James Bryce and the Perception of Power."

³⁴ Almond, "Power and Plutocracy in New York City" (Ph.D. dissertation, University of Chicago, 1939); Weinstein, *The Corporate Ideal in the Liberal State, 1900-1918* (Boston, 1968), chap. 4: "The Small Businessman as Big Businessman: The City Commission and Manager Movements"; and Kolko, "Brahmins and Business, 1870-1914: A Hypothesis on the Social Basis of Success in American History," in Kurt H. Wolff and Barrington Moore, Jr., eds., *The Critical Spirit: Essays in Honor of Herbert Marcuse* (Boston, 1967), 343-63.

have instead been forced to operate within a constraining framework of "institutions, national, regional, and local," political, legal, economic, social, and cultural. In Robert K. Lamb's view, each institution constitutes a base of power in its community, embodying accepted values and providing resources which its leaders may use to discipline even the wealthiest innovators.³⁵ Studies of economic development in nineteenth-century communities from Boston to Dodge City have found, as Lamb suggested, that economic and cultural conflicts often did limit the ability of entrepreneurs to respond to economic opportunity. These studies disagree as to the community characteristics most closely associated with dispersed power. In one of the most fully realized of these studies, Robert R. Dykstra argues that in late nineteenth-century frontier towns community conflict, if not a wide dispersion of power, was probably found most commonly in small, new, economically self-contained communities characterized by rapid in-migration, numerous voluntary associations, and culturally diverse populations.³⁶ But others see apparently similar small communities dominated by small elites, and most entrepreneurial studies support the view that power was more widely dispersed in cities that were older, larger, and economically more diverse.³⁷ By the end of the nineteenth century few decisions concerning economic development could be made and financed in a single community, but Maury Klein concludes that local interests retained substantial power even then.³⁸ Although opposition to entrepreneurs has usually been located among other business elites, Sigmund Diamond has shown how the seventeenth-century Virginia Company was forced to make fundamental concessions to its employees and how the pro-business press has continuously sought to justify entrepreneurs to the widest possible public.³⁹

Students of the history of social policy have been less concerned with the

³⁵ Lamb, "The Entrepreneur and the Community," in William Miller, ed., *Men in Business: Essays in the History of Entrepreneurship* (Cambridge, Mass., 1952), 91-119, esp. 94, and "Entrepreneurship and Community Development," in *Explorations in Economics: Notes and Essays Contributed in Honor of F. W. Taussig* (New York, 1936), 326-34.

³⁶ Dykstra, *The Cattle Towns* (New York, 1968), 371-78.

³⁷ Richard S. Alcorn finds "leadership" (officeholding) concentrated in a wealthy elite; see "Leadership and Stability in Mid-Nineteenth-Century America: A Case Study of an Illinois Town," *JAH*, 61 (1974): 685-702. For studies that find power more dispersed in larger, older communities, see, for example, Charles N. Glaab, *Kansas City and the Railroads: Community Policy in the Growth of a Railroad Metropolis* (Madison, Wisc., 1962), 43-55, 144, 171-72; Belcher, *Economic Rivalry between St. Louis and Chicago*, chap. 7; Charles N. Glaab and Lawrence H. Larsen, *Factories in the Valley: Neenah-Menasha, 1870-1915* (Madison, Wisc., 1969), 144, 153-54, 159-62, 216-19, 223-57; and Julius Rubin, *Canal or Railroad? Imitation and Innovation in the Response to the Erie Canal in Philadelphia, Baltimore, and Boston*, Transactions of the American Philosophical Society, n.s., vol. 51, pt. 7 (Philadelphia, 1961). Sam B. Warner, Jr. finds that the economic elite withdrew from political affairs as Philadelphia became a "Big City" after 1830; and he asserts that Boston's suburban development, at least, proceeded without direction from the economic elite; *The Private City: Philadelphia in Three Periods of Its Growth* (Philadelphia, 1969), esp. chap. 5, and *Streetcar Suburbs: The Process of Growth in Boston, 1870-1900* (Cambridge, Mass., 1962), esp. chaps. 2, 6. For two studies that find power tightly concentrated in cities of somewhat smaller size, see Leonard P. Curry, *Rail Routes South: Louisville's Fight for the Southern Market, 1865-1872* (Lexington, 1969), 7; and Herman R. Lantz, *A Community in Search of Itself: A Case History of Cairo, Illinois* (Carbondale, Ill., 1972), chap. 6.

³⁸ Klein, *The Great Richmond Terminal: A Study of Businessmen and Business Strategy* (Charlottesville, Va., 1970), 288.

³⁹ Diamond, "From Organization to Society: Virginia in the Seventeenth Century," *American Journal of Sociology*, 63 (1958): 457-75, and *The Reputation of the American Businessman* (Cambridge, Mass., 1955).

analysis of power and have offered more disparate assessments. Recent critics of schools, asylums, and other socializing and order-maintaining institutions have usually attributed dominant power over policy to economic elites, but other historians have found power much more dispersed in these areas after the Jacksonian period.⁴⁰ Nineteenth- and early twentieth-century social welfare and housing reform have been described as the product of elite politics,⁴¹ but historians of urban police forces in the nineteenth century have found them to be subject to multiple and diverse pressures, from political organizations and the poor as well as from various economic elites.⁴² Since few of these historians deal with policy in more than one area, it is possible that even those studies which find policy dominated by an economic elite have identified an elite of wealthy people who confined their attention to a single set of issues, and have not located a dominant power elite.

Historians of lower status groups have also offered at least qualified support for a dispersed power interpretation. Those who find immigrants or workers to be members of tightly knit communities possessed of their own culture and leadership often find them capable of making successful demands on the political system.⁴³ At the least, many historians now insist that such communities were able to prevent the larger society or the dominant class from disrupting treasured belief and social practice. This view is an important source of the now widely accepted "functional analysis" of politics, which stresses the support machines received in return for behind-the-scenes favors to the poor and the immigrants as well as to business and criminals.⁴⁴ By 1900, according to J. Joseph Huthmacher and others, machines in large cities were working openly and effectively for labor and welfare reform.⁴⁵ Herbert G.

⁴⁰ Compare Michael Katz, *The Irony of Early School Reform: Educational Innovation in Mid-Nineteenth-Century Massachusetts* (Cambridge, Mass., 1968), and David Rothman, *The Discovery of the Asylum: Social Order and Disorder in the New Republic* (Boston, 1971), with David B. Tyack, *The One Best System: A History of American Urban Education* (Cambridge, Mass., 1974), and Gerald N. Grob, *Mental Institutions in America: Social Policy to 1875* (New York, 1973).

⁴¹ Roy Lubove, *The Professional Altruist: The Emergence of Social Work as a Career* (Cambridge, Mass., 1965), chap. 6, and *The Progressives and the Slums: Tenement House Reform in New York City, 1890-1917* (Pittsburgh, 1962), chaps. 4-6.

⁴² Roger Lane, *Policing the City: Boston, 1822-1885* (Cambridge, Mass., 1967); and James F. Richardson, *The New York Police: Colonial Times to 1901* (New York, 1970).

⁴³ Oscar Handlin, *Boston's Immigrants* (Cambridge, Mass., 1941), chaps. 7, 8, and *The Uprooted* (rev. ed.; New York, 1951), chap. 8; Moses Rischin, *The Promised City: New York's Jews, 1870-1914* (Cambridge, Mass., 1962), chaps. 11-12; and Humbert S. Nelli, "John Powers and the Italians: Politics in a Chicago Ward," *JAH*, 58 (1970): 67-84. Although Rudolf J. Vecoli objects that Handlin fails to appreciate the differences among immigrant groups, he agrees that self-conscious immigrant communities could wield significant power; "Contadini in Chicago: A Critique of *The Uprooted*," *JAH*, 51 (1964): 404-17. For the most sophisticated statement of the point, see William F. Whyte, *Street Corner Society: The Social Structure of an Italian Slum* (Chicago, 1943).

⁴⁴ For the classic statement of the functional analysis of local politics, see Robert K. Merton, *Social Theory and Social Structure* (rev., enlarged ed.; New York, 1957), chap. 1: "Manifest and Latent Functions." For important historical applications of the approach, see, for example, A. Theodore Brown, *The Politics of Reform: Kansas City's Municipal Government, 1925-1930* (Kansas City, Mo., 1958); John Webb Pratt, "Boss Tweed's Public Welfare Program," *New York Historical Society Quarterly*, 45 (1961): 396-411; Seymour J. Mandelbaum, *Boss Tweed's New York* (New York, 1965); and Lyle W. Dorsett, *The Pendergast Machine* (New York, 1968). Eric McKittrick offers some revisions to Merton's *Social Theory and Social Structure*; see "The Study of Corruption," *Political Science Quarterly*, 72 (1957): 502-14.

⁴⁵ Huthmacher, "Urban Liberalism and the Age of Reform," *Mississippi Valley Historical Review*, 49 (1962): 231-41; and John D. Buenker, *Urban Liberalism and Progressive Reform* (New York, 1973).

Gutman also uses evidence of community solidarity in his argument that workers in smaller towns characterized by socially unified populations and common pre-industrial traditions often enjoyed sufficient middle class support to win significant concessions from factory owners during the last third of the nineteenth century.⁴⁶ Yet a number of studies demonstrate that lower status groups, whether resident in urban ghettos or in small factory towns, have rarely enjoyed such community stability or solidarity. For the period through 1880 there is now overwhelming evidence that most working people lived transient lives in transient, diverse communities.⁴⁷ If, as one group of scholars has recently argued, a strong consciousness of ethnic identity emerged only when people of a single cultural background and following a common occupation shared a crowded neighborhood, cultural identity can rarely have provided the basis for the exertion of power.⁴⁸ Although the ethnocultural interpretation of nineteenth-century voting patterns seems to argue to the contrary that the ethnic and religious values and sensitivities of large groups of people were potent factors in politics, Richard L. McCormick, noting that religious issues rarely figured in government policies, has suggested that "the most important message conveyed by ethnocultural analysis is . . . that grass-roots concerns are irrelevant to public policy making."⁴⁹

Several recent students of local politics, many of them influenced by Richard C. Wade, refuse to accept this conclusion. Building on the functional analysis of machine politics, these students suggest that power has long been quite widely dispersed at least in the larger cities; like those who have written on lower-status groups, they are not concerned with the origins of dispersion. In Wade's view, the older interpretation of urban politics in terms of bosses and reformers must be rejected precisely because it assigned too much power to a few prominent individuals; in fact, he argues, leaders of both types had to respect the wishes of their constituents. And their constituents differed: bosses

⁴⁶ Gutman, "Class, Status, and Community Power in Late Nineteenth-Century American Industrial Cities: Paterson, New Jersey—A Test Case," in Jaher, *Age of Industrialism in America*, 263–87. Gutman makes similar points in three other essays: "The Worker's Search for Power: Labor in the Gilded Age," in H. Wayne Morgan, ed., *The Gilded Age: A Reappraisal* (Syracuse, N.Y., 1963), 38–68; "An Iron Workers' Strike in the Ohio Valley, 1873–1874," *Ohio Historical Quarterly*, 68 (1959): 353–70; and "Trouble on the Railroads, 1873–1874: Prelude to the 1877 Crisis," *Labor History*, 2 (1961): 215–36. John T. Cumbler has shown, by contrast, that strong community support could also give industrial workers "a false sense of their power to effect their demands over organized capital"; "Labor, Capital, and Community: The Struggle for Power," *Labor History*, 15 (1974): 395–415.

⁴⁷ Stephan Thernstrom and Peter R. Knights, "Men in Motion: Some Data and Speculations about Urban Population Mobility in Nineteenth-Century America," *Journal of Interdisciplinary History*, 1 (1970): 7–35; and Peter R. Knights, *The Plain People of Boston, 1830–1860: A Study in City Growth* (New York, 1971), chaps. 3, 4, 6. For a post-1880 reconnaissance, see Stephan Thernstrom, *The Other Bostonians: Poverty and Progress in the American Metropolis, 1880–1970* (Cambridge, Mass., 1973), chap. 1.

⁴⁸ For a critique of the "ghetto interpretation" of immigrant experience in the United States, see Sam B. Warner, Jr., and Colin Burke, "Cultural Change and the Ghetto," *Journal of Contemporary History*, 4 (1969): 173–87; and Eugene P. Erickson, Richard N. Juliani, and William L. Yancey, "Emergent Ethnicity: A Review and Reformulation," unpublished paper of the Temple University Center for Studies of Metropolitan Problems, 1975. Richard S. Alcorn's findings that those who held leadership positions in Paris, Illinois between 1850 and 1855 were not only wealthier and engaged in higher-status occupations but also had lived in Paris for a longer period of time are also relevant to this point; "Leadership and Stability in Mid-Nineteenth-Century America," 701–02.

⁴⁹ McCormick, "Ethno-Cultural Interpretations of Nineteenth-Century American Voting Behavior," *Political Science Quarterly*, 89 (1974): 371.

represented the central wards, whose residents were poor and frequently belonged to immigrant stock, while the reformers usually spoke for the Protestant, middle class suburbs. Thus, the master conflict in urban politics since at least the last third of the nineteenth century has been a conflict of “the periphery against the city.”⁵⁰ This conflict, Wade apparently believes, has been more fundamental than any cross-cutting cleavage over religion or ethnicity among the poor, or between businessmen and reformers. As Dykstra has pointed out, Wade’s is “fundamentally a class-conflict model.”⁵¹ But the studies that Wade himself cites as testing or illustrating his model all find that the successful political regime has been forced to seek accommodation, not conflict; that successful reformers,⁵² like successful bosses,⁵³ have offered significant services to the poor as well as to the well to do.

Among those who detect changes in the distribution of power since 1800, Samuel P. Hays has advanced by far the most comprehensive argument for the fifth general interpretation of the course of power in American communities, an interpretation which sees increasing inequality since the 1890s. Relying largely on the evidence of officeholding and on the intentions of municipal reformers, Hays suggests that, although power was tightly held by the economic and social elites of American cities before 1850, these elites remained in close touch with—and, he implies, were to some degree constrained by—the rest of the population. As the cities grew larger and less integrated socially under the impact of industrialization, power was dispersed as decentralized, ward-based local governments replaced town meetings. Toward the end of the nineteenth century, he observes, “working-class wards frequently elected workingmen,” many of whom were, like their constituents, Catholics and immigrants. But between the late 1890s and the Great Depression, Hays argues, the largest business interests (“manufacturers, downtown merchants, central-city bankers, and managers of central-city property”), joined by such professionals as educational administrators, public health doctors, civil engineers, and architects, and by the “urban upper class,” successfully worked for “integration and centralization in decision-making” and for the exclusion of lower status groups from public office. The depression temporarily brought some workingmen back into office, but the rise of autonomous suburbs and the development of institution-based politics within the large cities after the Second World War continued the process whereby power

⁵⁰ Wade, “The City in History: Some American Perspectives,” in Werner Z. Hirsch, ed., *Urban Life and Form* (New York, 1963), 72, *passim*, and “An Agenda for Urban History,” in Herbert J. Bass, ed., *The State of American History* (Chicago, 1970), 50–58.

⁵¹ Robert R. Dykstra, “Stratification and Community Political Systems: Historians’ Models,” in Allan G. Bogue, ed., *Emerging Theoretical Models in Social and Political History* (Beverly Hills, Calif., 1973), 93.

⁵² Melvin G. Holli, *Reform in Detroit: Hazen S. Pingree and Urban Politics* (New York, 1969); and Allen F. Davis, *Spearheads for Reform: The Social Settlements and the Progressive Movement* (New York, 1967). For other relevant studies, see Brown, *Politics of Reform: Kansas City’s Municipal Government*; and James B. Crooks, *Politics and Progress: The Rise of Urban Progressivism in Baltimore, 1895 to 1911* (Baton Rouge, 1968).

⁵³ Zane L. Miller, *Boss Cox’s Cincinnati: Urban Politics in the Progressive Era* (New York, 1968); and Dorsett, *The Pendergast Machine*. Also see Harold F. Gosnell, *Boss Platt and His New York Machine: A Study of the Political Leadership of Thomas C. Pratt, Theodore Roosevelt, and Others* (reprint ed.; New York, 1969).

was increasingly concentrated in the upper third of the “social and political order.”⁵⁴

Most of those who have described changes in the distribution of power have instead belonged to a sixth group, arguing that power became increasingly dispersed between about 1830 and 1850 and again between about 1890 and 1914. As Lee Benson recently observed, the “dominant historical view” holds “that American society (broadly conceived) became significantly more democratic during the Middle Period.”⁵⁵ Restrictions on the suffrage and qualifications for holding office were significantly reduced during the period, and mass participation in politics—through voting and active membership in political parties—greatly increased. At the same time, it is generally agreed that political and social values in the United States became more egalitarian.⁵⁶ In the larger cities, according to several writers, patrician elites were withdrawing from local politics by the 1850s, leaving the field to narrow-minded, unscrupulous politicians and self-interested petty entrepreneurs.⁵⁷ For the Progressive period, Arthur S. Link has offered the most succinct statement of the Progressives’ claim that they served democratic purposes. Citing the direct primary, short ballot, initiative, referendum and recall, and the commission and city manager movements, he argues that these new political institutions facilitated “popular rule and responsible and enlightened government.”⁵⁸ If professional administrators who entered municipal government during this era often came from affluent social backgrounds, George E. Mowry observes that they introduced “gas and water socialism,” increased protections for women and children, and sponsored significant labor, housing, and public health reform.⁵⁹ Yet it is by no means certain that the affluent entirely displaced the workingmen after 1900. Bosses Murphy, Cox, Crump, Pendergast, Lomasny, and Hague and Mayor Daley all flourished during and after the Progressive Era.⁶⁰ In the most influential of several studies of office-holding, Robert A. Dahl concluded that “in the course of the past two centuries, New Haven has gradually changed from oligarchy to pluralism” as “patricians” (1784–1842), entrepreneurs (1842–1900), and “ex-plebes” (after 1900) rising from the lower strata through political machines successively ruled the city.⁶¹

⁵⁴ Hays, “The Politics of Reform in Municipal Government in the Progressive Era,” *Pacific Northwest Quarterly*, 55 (1965): 157–69, and “Changing Political Structure of the City,” 6–38.

⁵⁵ Benson, “Middle Period Historiography: What Is to Be Done?” in George A. Billias and Gerald N. Grob, eds., *American History: Retrospect and Prospect* (New York, 1971), 177, and *The Concept of Jacksonian Democracy: New York as a Test Case* (Princeton, 1961), 336.

⁵⁶ Benson, “Middle Period Historiography,” 184, and *The Concept of Jacksonian Democracy*, 336.

⁵⁷ Persons, *Decline of American Gentility*, 131–40; and Warner, *The Private City*: Philadelphia, 98.

⁵⁸ Link, *American Epoch: A History of the United States since the 1890's* (New York, 1955), 84–86.

⁵⁹ Mowry, *The Era of Theodore Roosevelt and the Birth of Modern America, 1900–1912* (New York, 1958), 66–67.

⁶⁰ See Nancy J. Weiss, *Charles Francis Murphy, 1858–1924: Respectability and Responsibility in Tammany Politics* (Northampton, Mass., 1968); Harold Zink, *City Bosses in the United States* (Durham, N.C., 1930); and Edward C. Banfield and James Q. Wilson, *City Politics* (Cambridge, Mass., 1963).

⁶¹ Dahl, *Who Governs? Democracy and Power in an American City*, 11. In reviewing modern European history in hopes of finding a paradigm for the United States, Charles Tilly has suggested that the industrialization and urbanization of the labor force has produced three successive changes in the distribution of power. In the short run, the transformation of society disoriented the workingmen, reducing both their power and

CONFLICTS AMONG THESE VARIOUS INTERPRETATIONS of the history of power in American communities stem far more from the historians' choices of approach and subject matter and from their findings in specific communities than from their allegiance to particular political views or historical schools. Defenders of the business system agree with New Left revisionists that power was often dispersed, while both radical social critics and elitists seeking to inculcate virtue in the next generation of leaders agree that power has been held closely by a few. The most widely shared tendency is for each historian to ascribe a significant measure of power to the individual or group he has studied most closely. When historical attention was largely devoted to elites, they appeared to dominate; the more recent shift of attention to other groups makes it clear that no group has been entirely powerless and that no elite has entirely been able to ignore preferences of others in its community. Yet it is not at all clear how power has been distributed among the various groups and social strata; very few studies have compared elites with one another and with others in a single community or employed an approach which facilitates comparisons from place to place and time to time under varying institutional arrangements. There is a general consensus that we need to assess the relative power of economic, social, and political elites, of nonelite economic interests, of the several income and occupational classes, and of religious, ethnic, and racial subgroups. Since few studies of small, rural, or southern communities have considered the question of power, we have only a preliminary sense of the effect of urbanization and region on the distribution of power.

Most historians do agree that economic—or economic and social—elites, after dominating their cities early in the nineteenth century, faced particularly vigorous challenges during two periods, 1830–50 and 1890–1914; some would add that the elites responded vigorously during the 1870s and after 1914.⁶² One of the most important factors in determining the success of elites was the degree of their unity; but, although it seems most likely that elites became increasingly fragmented as communities grew older, larger, and economically more complex, only the entrepreneurial studies have yet investigated this point, and they deal only with a very restricted range of issues. Other studies of economic and social elites have defined elite membership in such diverse, and often uncertain, ways as to make it impossible to form a clear view of the history of the power of elites, despite the great historical attention devoted to them. Uncertainty regarding membership and functions similarly precludes ascertaining the effect of voluntary associations on the distribution of power,

that of traditional local ruling elites while increasing the power of the national state. In the long run, urbanization permitted large groups of workingmen to associate with one another in an effort, often violent, to regain influence. In the very long run, the new associations succeed in achieving recognition and influence. The second two parts of this scheme closely follow the "progressive" interpretation of protest movements in the United States since the 1880s. See Tilly, "Collective Violence in European Perspective," in Hugh Davis Graham and Ted Robert Gurr, eds., *Violence in America: Historical and Comparative Perspectives* (New York, 1969), 4–42. William A. Gamson offers some support for Tilly's approach, especially on the role of violence; *The Strategy of Social Protest* (Homewood, Ill., 1975).

⁶² Mandelbaum, *Boss Tweed's New York*, chaps. 8, 9; and Weinstein, *The Corporate Ideal in the Liberal State*, chap. 4.

though again it seems most likely that the increased numbers of associations have tended to disperse power—at least among those who have joined—and that in the twentieth century the less affluent—if not the poor—have increasingly formed their own associations.

During both the Jacksonian and the Progressive Eras changes in the suffrage, the methods of voting, the party organizations, and the structure and powers of local governments are alleged to have had significant consequences for the distribution of power. But the nature of the consequences is sharply disputed, for reasons similar to those which have provoked the social scientists' dispute over the "second face" of power and over the "rule of anticipated reactions."⁶³ One part of the historical dispute concerns deference. J. R. Pole speculated that deferential attitudes "collapsed in ruin in the upheaval of Jacksonian democracy," but men of wealth and standing continued to hold local office until after 1860. Nor is it clear how deferential attitudes affect the distribution of power: a deferential voter may have been giving up power to a dominating elite, or he may have been selecting officials who would act as he preferred. In either case, he may well have remained ready to protest or riot when official policy diverged from standards he considered proper or just. Few studies have fully and explicitly examined deference in the cities of nineteenth-century America, although studies of two small New England cities do suggest that, while deferential traditions persisted into the 1870s or 1880s, they dissolved as immigration and population growth transformed the cities into fragmented multicultural communities.⁶⁴ But we do not know whether immigrant groups then began to increase their power, because other historical disputes concern the possibilities that political corruption and increased control of information may have replaced deference as "hidden" sources of elite power in the years after 1865 and that voluntary associations and state and national government agencies may have captured many of the functions and powers earlier held by local governments.

In short, the problems for research are now far more evident than are acceptable generalizations about the history of power in American communities. We cannot construct a theory to account for historical variations in the distribution of power until we have a much firmer grasp on the variations to be explained. Our incomplete knowledge of the history of power helps account for the fact that such community characteristics as youth and age, smallness and bigness, anomie and a strongly shared sense of group identity have all been said, by some historians, to promote the concentration of power and, by others, to encourage its dispersal. The most promising hypothesis—suggested by recent work on the experience of the poor and the minorities as

⁶³ For discussions of the "Second Face of Power" and "Rule of Anticipated Reaction," see J. R. Pole, "Historians and the Problems of Early American Democracy," *AHR*, 67 (1961-62): 626-46; Peter Bachrach and Morton S. Baratz, "Two Faces of Power," *American Political Science Review*, 56 (1962): 947-52; Carl J. Friedrich, *Man and His Government* (New York, 1963), 199-215; and Nagel, *The Descriptive Analysis of Power*, 16-34.

⁶⁴ Pole, "Historians and the Problems of Early American Democracy," 646. And see Frisch, *Town into City: Springfield, Massachusetts*; and Estelle F. Feinstein, *Stamford in the Gilded Age: The Political Life of a Connecticut Town, 1868-1893* (Stamford, Conn., 1973).

well as by earlier studies of politics and entrepreneurship and consistent with the recent work of such sociologists as Michael Aiken—is that power has become increasingly dispersed as American communities have become larger, as their economies have become more specialized and more dependent on expertise and coordination, and as their populations have become more diverse. But at present this can only be a hypothesis with several vigorous rivals.

TWO CRITERIA ARE PARTICULARLY RELEVANT to the selection of a method for resolving the inconsistencies and contradictions in our knowledge of the history of community power.⁶⁵ The method adopted should address the main disputes both about the distribution of power and about the relation between power and the several economic, social, institutional, and ideological variables; to do so, it should measure power independently of economic or social standing. The method should also be applicable to communities of varying size and institutional arrangements, over the changing circumstances of the past one hundred and seventy years. In addition, the method should not determine results, should permit historians to compare their results with those reached by political and social scientists, and should be applicable to historical sources. The method should also permit comparison of American communities with those of other societies—many generalizations about the history of power in the United States do make implicit comparisons with other cultures. Robert Dahl and several other political scientists have argued that the distribution of power in any community can best be studied through the “careful examination of a series of concrete decisions.”⁶⁶ Modified in the light of the extensive discussion of method among sociologists and political scientists and adapted for historical research, this approach best satisfies the criteria outlined here.

To study power through decisions is to reject three other indicators of power which have been used by a number of social scientists and historians: the holding of office, the possession of a reputation for power, and the ability to benefit from the outcome of a decision or from maintenance of the *status quo*. Since it appears to be possible to examine position-holding, reputation, and at least the financial aspects of public policy systematically, quantitatively, and relatively quickly, these indicators hold a real attraction for historians. But none of them approaches the measurement of power directly, and all of them entail assumptions about the relationship between power and other variables. A critical review of these approaches to power will indicate why they produce incomplete results, and why the analysis-of-decisions approach is the most satisfactory.

⁶⁵ For other discussions, definitions, and methods for the historical study of community power, see Dykstra, *The Cattle Towns*, app. B; Richard Gillam, *Power in Postwar America* (Boston, 1971); Lee Benson, “Political Power and Political Elites,” in Benson *et al.*, *American Political Behavior: Historical Essays and Readings* (New York, 1974), 281–310; and Carl V. Harris, “The Underdeveloped Historical Dimension of the Study of Community Power Structure,” *Historical Methods Newsletter*, 9 (1976): 195–201.

⁶⁶ Dahl, “A Critique of the Ruling Elite Model,” 463–69.

Most explicit generalizations about the history of power distribution in American communities have been based on studies of the characteristics of officeholders. Nearly all of the studies of officeholding so far published support the view that, while local economic elites held most high municipal offices until the 1840s, professional politicians moved into office as communities became larger and economically more complex. Thus, we are told that professional politicians have continuously held the mayor's office in Chicago since 1868, in New Haven since 1900, in Nashville since 1909, and in Ypsilanti, Lansing, and other middle-sized communities at least since the 1920s. Less complete studies suggest that professional politicians began serving as mayor even earlier in Philadelphia and New York. Until the 1920s, some of these successful politicians were the scions of wealthy families, but in most communities they have increasingly derived from the middle or lower strata, moving up either through the party organizations, or, especially since the 1930s, through career ladders in technical fields relevant to municipal administration.⁶⁷ Members of the economic elites have, however, maintained or increased their participation in local voluntary associations. Smaller communities have lagged behind larger ones in these shifts; smaller communities in which the main economic enterprises are branch plants of national corporations have followed more closely than have those whose largest employers have remained independent.⁶⁸

⁶⁷ Almond, "Power and Plutocracy in New York City" (1865-1930); Robert O. Schulze, "The Role of Economic Dominants in Community Power Structure," *American Sociological Review*, 23 (1958): 3-9, and "The Bifurcation of Power in a Satellite City," in Morris Janowitz, ed., *Community Power Systems* (Glencoe, Ill., 1961), 19-80 (on "Cibola"—Ypsilanti, Michigan, 1823-1954); Wallace S. Sayre and Herbert Kaufman, *Governing New York City: Politics in the Metropolis* (New York, 1960), 679-89 (1898-1959); Dahl, *Who Governs? Democracy and Power in an American City*, 11-86 (New Haven, 1784-1954); Donald A. Clelland and William H. Form, "Economic Dominants and Community Power: A Comparative Analysis," *American Journal of Sociology*, 69 (1964): 511-21 (on "Wheelsburg"—Lansing, Michigan, 1860-1959); John H. Lindquist, "Socioeconomic Status and Political Participation," *Western Political Quarterly*, 17 (1964): 608-14, and "An Occupational Analysis of Local Politics: Syracuse, New York, 1880-1959," *Sociology and Social Research*, 49 (1965): 343-54; Theodore J. Lowi, *At the Pleasure of the Mayor: Patronage and Power in New York City, 1898-1958* (Glencoe, Ill., 1964); Donald S. Bradley and Mayer N. Zald, "From Commercial Elite to Political Administrator: The Recruitment of the Mayors of Chicago," *American Journal of Sociology*, 71 (1965): 153-67; Mayer N. Zald and Thomas A. Anderson, "Secular Trends and Historical Contingencies in the Recruitment of Mayors: Nashville as Compared to New Haven and Chicago," *Urban Affairs Quarterly*, 3 (1968) (Nashville, 1806-1963); D. Clayton James, *Antebellum Vachez* (Baton Rouge, 1968), 93-96 (1803-61); Michael F. Holt, *Forging a Majority: The Formation of the Republican Party in Pittsburgh, 1848-1860* (New Haven, 1968), 322-26, 349-54, 361-66, 376-81; Carl V. Harris, "Economic Power and Politics: A Study of Birmingham, Alabama, 1890-1920" (Ph.D. dissertation, University of Wisconsin, 1970), chap. 5 (officeholding, 1870-1953); Michael H. Frisch, "The Community Elite and the Emergence of Urban Politics, Springfield, Massachusetts, 1840-1880," in Stephan Thernstrom and Richard Sennett, eds., *Nineteenth-Century Cities: Essays in the New Urban History* (New Haven, 1969), 277-96, and *Town into City: Springfield, Massachusetts*, 40-41, 241-46; Edward Pessen, "Who Governed the Nation's Cities in the 'Era of the Common Man'?" *Political Science Quarterly*, 87 (1972): 591-614 (Boston, Brooklyn, New York, and Philadelphia, 1825-50); Feinstein, *Stamford in the Gilded Age*, chaps. 2, 9 (1865-93); Peter H. Rossi and Alice S. Rossi, "An Historical Perspective on the Functions of Local Politics," in Daniel N. Gordon, ed., *Social Change and Urban Politics: Readings* (Englewood Cliffs, N.J., 1973), 49-64 ("Bay City," Massachusetts, 1760-1957); and Katz, *The People of Hamilton, Canada West, 176-88* (years around 1851). For collections of the best of the journal articles cited here, see Gordon, *Social Change and Urban Politics*; and Aiken and Mott, *The Structure of Community Power*.

⁶⁸ Clelland and Form, "Economic Dominants and Community Power"; Rossi and Rossi, "An Historical Perspective on the Functions of Local Politics"; and Roland J. Pellegrin and Charles H. Coates, "Absentee-Owned Corporations and Community Power Structure," *American Journal of Sociology*, 61 (1956): 413-19.

This long-term trend is striking, but beyond illustrating the association between economic development and increasing division of labor, its meaning is unclear. When economic elites relinquished office, they may have found it more cumbersome to exert power. But to hold office is not necessarily to hold power.⁶⁹ It is true that public or voluntary association office, like wealth or social prestige, may be a valuable resource to its possessor in any effort to exert power. Evidence of party organization and voter participation indicates that local offices were often eagerly sought in nineteenth-century communities. But the possessor of resources may lack the ability or the will to use them effectively; not every president has made good use of his "bully pulpit," and officeholders often feel constrained to please their constituents. Moreover, the nineteenth century had an entire vocabulary for questioning the power of officeholders: Did the mayor owe his position, and his allegiance, to a boss, a kingmaker—a Warwick? Was he controlled by a power behind the throne, a grey eminence, a kitchen cabinet? Was he a mere *locum tenens*, a placeholder acting for an unseen principal? It was often said that capitalists, too busy and too shrewd to waste time with the front man, made their deals with the "ring" behind the scenes. Even when a mayor was his own man, he might well find, especially between the 1850s and the 1880s, that he had little authority over city officials and that many local activities were under the control of state or county commissioners and not within his purview at all. Between 1857 and 1870, for instance, the mayor of the City of New York had no control over the city's police department; at other times the mayor, the county sheriff, the state governor, and federal authorities have shared control over the use of legitimate force. Insofar as nineteenth-century communities obeyed the strictures of Manchester Liberalism—or of Jacksonian Democracy—many substantive issues fell outside the responsibility of all their governments. For the twentieth century, it is not clear whether the relative power of local governments increased or declined at the very time when members of local economic elites stopped holding high office. Even when officeholders do exercise power, they do not necessarily act on behalf of the group to which they belong; hence, knowledge of their social origins is not adequate evidence of the distribution of power within a society. No study of community can ignore formal governments, but the power of government officials must always be defined and

⁶⁹ On the implications of the "bifurcation of power" between "economic dominants" and professional political officeholders, see C. Wright Mills and Melville J. Ulmer, *Small Business and Civic Welfare: Report of the Smaller War Plants Corporation to the Special Committee to Study Problems of American Small Business*, Senate doc. no. 135, 79th Congress, 2d Session (Washington, 1946), pt. 4, reprinted in Aiken and Mott, *The Structure of Community Power*, 124–54; Schulze, "The Bifurcation of Power in a Satellite City"; Peter H. Rossi, "The Organizational Structure of an American Community," in Amitai Etzioni, ed., *Complex Organizations: A Sociological Reader* (New York, 1961), 301–12; Clelland and Form, "Economic Dominants and Community Power"; Robert Mills French, "Change Comes to Cornucopia—Industry and the Community," in French, ed., *The Community: A Comparative Perspective* (Itasca, Ill., 1969), 392–407, and "Economic Change and Community Power Structure: Transition in Cornucopia," in Aiken and Mott, *The Structure of Community Power*, 181–89; and Paul E. Mott, "The Role of the Absentee-Owned Corporation in the Changing Community," in Aiken and Mott, *The Structure of Community Power*, 170–79. Mott's article provides an important critique of the work both of Schulze and of Clelland and Form. Along with Mott, Pelligrin and Coates argue that executives in absentee-owned corporations exert more influence than Schulze recognized; "Absentee-Owned Corporations and Community Power Structure," 413–19.

demonstrated. Many of these considerations also make it difficult to define "community"; since city governments are mere legal creatures of their states, they have never had a monopoly of physical force and have never been able to settle most policy issues. For large cities, at least, it seems most appropriate to include the entire metropolitan area in the "community" and to consider the power of higher levels of government over local affairs.⁷⁰

A few historians have recently suggested that we might meet some of these objections by assuming that the officers of political parties or other voluntary associations shared power with public officials.⁷¹ Again, this is to assume what must be demonstrated. Voluntary associations and their members may indeed have enjoyed power in the nineteenth century similar to that which recent studies suggest they enjoyed in the mid-twentieth. Yet all voluntary associations are not equal today; eighty years ago the Chamber of Commerce of the State of New York exerted stronger power over a much wider scope of affairs in New York City than even the Board of Trade and Transportation, to which many business leaders and reputed millionaires belonged.⁷² Nor can we assume that the powers and functions of voluntary associations have remained constant for one hundred and fifty years; certainly the powers of specific kinds of associations, such as ethnic and labor organizations, have changed dramatically during this period. Many sociologists, moreover, seeking to account for the disappearance of members of economic elites from the roster of municipal officials, argue that since the end of the nineteenth century these elites have increasingly chosen to exert power through business, charitable, and "civic" organizations.⁷³

In an effort to get behind the official façade, some sociologists have asked carefully selected sets of citizens, "Who are the most powerful people in your community?"⁷⁴ Analogously, historians sometimes use evidence of a man's reputation for power as evidence that he was indeed powerful.⁷⁵ It is certainly

⁷⁰ On state control of a municipal police force, see James F. Richardson, "Mayor Fernando Wood and the New York Police Force," *New York Historical Society Quarterly*, 50 (1966): 5-40; and Samuel A. Pleasants, *Fernando Wood* (New York, 1948), 66-83. Kenneth T. Jackson has established the significance of annexation in the nineteenth century; "Metropolitan Government versus Political Autonomy: Politics on the Crabgrass Frontier," in Jackson and Stanley K. Schultz, *Cities in American History* (New York, 1972), 442-62, and "Urban Deconcentration in the Nineteenth Century: A Statistical Inquiry," in Leo F. Schnore, ed., *The New Urban History: Quantitative Explorations by American Historians* (Princeton, 1975), 110-42.

⁷¹ Edward Pessen, "The Role of the Rich and Elite in Local Voluntary Associations," in Pessen, *Riches, Class, and Power before the Civil War* (Lexington, Mass., 1973), 251-80; and Walter S. Glazer, "Participation and Power: Voluntary Associations and the Functional Organization of Cincinnati in 1840," *Historical Methods Newsletter*, 5 (1972): 151-68.

⁷² See my "Participation in Major Decisions in New York City, 1890-1900," esp. chaps. 5, 7.

⁷³ Rossi and Rossi, "An Historical Perspective on the Functions of Local Politics"; and Mott, "The Role of the Absentee-Owned Corporation."

⁷⁴ For the most influential of the reputational studies by a sociologist, see Floyd Hunter, *Community Power Structure: A Study of Decision Makers* (Chapel Hill, N.C., 1953). For critiques, see among others Dahl, "A Critique of the Ruling Elite Model"; and Raymond E. Wolfinger, "Reputation and Reality in the Study of Community Power," *American Sociological Review*, 25 (1960): 636-44. For the most persuasive defense, see William A. Gamson, "Reputation and Resources in Community Politics," *American Journal of Sociology*, 72 (1966): 121-31.

⁷⁵ Ernest R. May, "An American Tradition in Foreign Policy: The Role of Public Opinion," in William H. Nelson, ed., *Theory and Practice in American Politics* (Chicago, 1964), and *American Imperialism: A Speculative Essay* (New York, 1968).

important to know what people think about the distribution of power, since a reputation for power can be a valuable resource. Our best evidence indicates that economic and social elites retained great confidence in their own power at least until the end of the nineteenth century, even though they were then giving up municipal offices.⁷⁶ But evidence concerning reputations cannot by itself show how power was in fact distributed. As usually applied, the study of reputations for power assumes one of the things which is to be shown: that a small elite exercised power over a wide scope of affairs. It relies on indirect evidence, derived from informants who may report hearsay, conjecture, or statements they know to be false, and whose criteria for ascribing power may vary both from one informant to another and from criteria acceptable to the investigator. Each source should be expected to exaggerate the power of his or her associates, yet on occasion to attribute defeat by a majority to the machinations of backroom bosses or faceless bureaucrats. Nineteenth-century newspapers, which may be subjected to content analysis for evidence about reputations, were notoriously political and idiosyncratic in their news columns as well as in their editorials. Historical sources, at least as much as the subjects of contemporary interviews, disproportionately offer evidence on the reputations of economic and social elites; hence, the reputational approach does not measure power independently of wealth and status.

In the last several years, some social scientists have shifted their attention from the participants in the decision-making process to the beneficiaries of its results, asking, for example, who benefits from decisions concerning taxation and expenditure.⁷⁷ Fiscal data are available for long periods of time for many communities since 1800; as the Hollingsworths have shown, such data can be collected, put on a comparative basis, and analyzed to produce very suggestive findings.⁷⁸ But since a decision may fail to accomplish its author's intent, results do not necessarily indicate power. A powerful man may, like Lear, unintentionally act so as to harm himself, while those who benefit from a given state of affairs or a specific decision may be more fortunate than powerful. Indeed, they may even be unfortunate, like those who inherit conspicuous privilege on the eve of a revolution.

In contrast to these approaches, the study of power through the examination of a series of major decisions permits direct observation of the exercise of power. The comparison of several decisions makes it possible to determine whether a person or a group which dominates one decision has power over

⁷⁶ Hammack, "James Bryce and the Perception of Power."

⁷⁷ Terry N. Clark, "Community Structure, Decision-Making, and Urban Renewal in 51 American Communities," *American Sociological Review*, 33 (1968): 576-93; Robert L. Lineberry and Edmund P. Fowler, "Reformism and Public Policies in American Cities," *American Political Science Review*, 61 (1967): 701-16; Louis H. Masoti and Don R. Bowen, "Communities and Budgets: The Sociology of Municipal Expenditures," *Urban Affairs Quarterly*, 2 (1965): 39-58; and Michael Aiken, "The Distribution of Community Power: Structural Bases and Social Consequences," in Aiken and Mott, *The Structure of Community Power*, 487-525.

⁷⁸ J. Rogers Hollingsworth and Ellen Jane Hollingsworth, "Expenditures in American Cities," in William O. Aydelotte et al., eds., *The Dimensions of Quantitative Research in History* (Princeton, 1972), 347-89; and Terry N. Clark, "The Irish Ethic and the Spirit of Patronage," *Ethnicity*, 2 (1975): 305-59.

other aspects of public life, permitting conclusions about the scope of its power.⁷⁹ That a decision is made assures that alternatives, some possibly favored by members of the community, were rejected. Analysis of decisions thus permits conclusions about the relative ability of participants to overcome opposition—conclusions about the strength and extent of their power.⁸⁰ Finally, the analysis-of-decisions approach makes few assumptions about the central problems in community power research, the relationships between power and such other variables as wealth, occupation, status, and the changing forms and powers of local government.

Since no historian can study all decisions taken in a large community during even a limited period of time, it is necessary to be selective.⁸¹ This cannot be an easy, or a noncontroversial, task, but it is a task familiar to historians. The most efficient approach involves choosing major decisions that have the following characteristics: They affect the largest number of people either by redistributing the largest amounts of valued resources, by changing the way policies are established and administered, or by affecting the values, beliefs, and information which constitute the climate of opinion within which proposals are considered.⁸² They are viewed as the most important by contemporary and later experts, by reputedly powerful groups in the community, or by large segments of the community's population.⁸³ They take a long time to resolve. They are not necessarily resolvable within the community's local government or even within the "community" itself. The most important decisions may be "nondecisions," which keep ideas off the public agenda or prevent the publication of information.⁸⁴ Analysis of decisions of this sort, from three or four distinct areas of community life (for example, decisions which are primarily economic, cultural, and political) permits conclusions to be drawn as to the existence or absence of a single, unified, dominant power elite. No power elite could ignore or permit itself to be excluded from such important decisions. At the same time, because they evoke the widest participation and produce the fullest historical sources, major decisions provide the best opportunity to evaluate the power of rising elite and nonelite groups. Since this approach makes no assumptions about the powers of officials or the significance of government forms and boundaries, a series of studies carried out on these lines would permit comparisons over time and space, among communities with differing institutions, located in different regions or nations.

⁷⁹ Dahl, "The Concept of Power," 201–15.

⁸⁰ *Ibid.*

⁸¹ For attempts to examine all decisions taken in particular communities over long periods of time, see Sayre and Kaufman, *Governing New York City*; and Harris, "Economic Power and Politics: A Study of Birmingham, Alabama."

⁸² Polsby, *Community Power and Political Theory*, 96.

⁸³ Dahl, *Who Governs? Democracy and Power in an American City*, 124–25, 148–49; and Linton C. Freeman *et al.*, "Locating Leaders in Local Communities," *American Sociological Review*, 28 (1963): 791–98.

⁸⁴ Bachrach and Baratz, "The Two Faces of Power," 244; and Norton E. Long, "Political Science and the City," in Leo Schnore, ed., *Social Science and the City: A Survey of Urban Research* (New York, 1968), 250. For a fuller discussion of these points, see my "Participation in Major Decisions in New York City, 1890–1900," chap. 2.

Peter Bachrach, Morton Baratz, and other neostratificationists have argued that to focus sharply on decision making may be to ignore significant aspects of the distribution of power. These scholars insist that it is necessary to consider how, through institutions and values, "some issues are organized into politics while others are organized out" and how some actors may be able to "define the situation" so that proposals unfavorable to themselves seem inappropriate, irresponsible, or inconceivable.⁸⁵ They also point out that an actor may be influenced indirectly as well as directly, through an intermediary or by anticipating another's actions. Steven Lukes has recently sought to revive what amounts to the issue of false consciousness, asserting that some participants in a decision might favor an outcome harmful to themselves through ignorance of consequences or a mistaken notion of their self-interest.⁸⁶ These aspects of power must certainly be taken into account. But since they constitute resources which may or may not be used effectively by their potential beneficiaries in particular circumstances, evidence of their importance can best be sought through the analysis of decisions.⁸⁷

Assessing the power of participants in decision-making is another difficult task of the sort historians frequently confront. The chief requirements are determining the implications of each decision and its possible alternatives, identifying the individuals and groups likely to be affected, ascertaining their preferences, discovering how they acted, and evaluating the degree to which the outcome met each possible participant's preferences.⁸⁸ The necessary sources are available, at least for many late-nineteenth-century communities in the United States. For such different communities as Stamford, Connecticut and New York City in that period, it has been possible to determine the preferences and both the public and behind-the-scenes actions of large proportions of the participants in major decisions and to learn a great deal

⁸⁵ Peter Bachrach and Morton S. Baratz, "Decisions and Nondecisions: An Analytical Framework," *American Political Science Review*, 57 (1963): 632-42, and "The Two Faces of Power"; Long, "Political Science and the City"; Terry N. Clark, "The Concept of Power: Some Overemphasized and Underrecognized Dimensions—An Examination with Special Reference to the Local Community," *Southwest Social Science Quarterly*, 48 (1967): 271-86; and E. E. Schattschneider, *The Semi-Sovereign People* (New York, 1960). For critiques of much of this work, see Raymond E. Wolfinger, "Nondecisions and the Study of Local Politics," *American Political Science Review*, 65 (1971): 1063-80; and the subsequent exchange between Wolfinger and Frederick W. Frey, *ibid.*, 1081-1104. Also see Geoffrey Debnam, "Nondecisions and Power: The Two Faces of Bachrach and Baratz," with a comment by Bachrach and Baratz and a rejoinder by Debnam, *American Political Science Review*, 69 (1975): 889-907. As Debnam insists, strictly speaking there are no "nondecisions": decisions to restrict the distribution of information, distribute false information, reinforce deferential attitudes, exclude certain groups from participation, or establish procedures that aid other groups are frequently made and should be studied just as other decisions are studied.

⁸⁶ Lukes, *Power: A Radical View*, 21-35.

⁸⁷ Nagel, *The Descriptive Analysis of Power*, esp. chap. 8.

⁸⁸ If the preferences and actions of enough of the possible participants can be discovered and if each decision can be broken down into a large number of subdecisions, it may even be possible to measure the relations between preferences and outcomes quite precisely through a form of the multiple regression statistical method known as path analysis. In view of the limited information available for even the best-documented historical events, however, it is likely that path analysis is too precise a tool and that use of the method would produce excessively abstract conclusions expressed in unjustifiably precise terms. When adequate sources on the actions of participants are not available, it is possible, as Carl Harris has shown, to infer a great deal about the distribution of power by systematically comparing preferences or probable preferences with the outcomes of decisions. See his "Economic Power and Politics." This approach is, however, unavoidably subject to the limits which apply to any analysis of beneficiaries.

about the preferences and actions of many who did not belong to the local economic elites.⁸⁹

In the New York state portion of the New York metropolitan region, these sources show, the ability to exert power by influencing the outcome of major decisions was largely concentrated in a number of distinct economic and political elites, which usually confined their efforts to a limited scope of public affairs.⁹⁰ Membership in these elites was always shifting; and, in their efforts to influence decisions, the elites themselves formed shifting alliances, bargained within one another and sometimes made concessions to obtain the support of other groups and of wider nonelite publics. The very poor, the most recent immigrants, and most of the unskilled, skilled, and white-collar workers had little direct representation among the decision-makers, although their votes, petitions, demonstrations, and strikes or threatened strikes helped to determine who did participate and under what conditions. In specific instances, a number of middle-class special interest groups, including the well-organized public school teachers, Bronx Protestant ministers, neighborhood bankers and merchants, the Brooklyn social elite, and Catholic and Jewish fraternal and religious organizations, exerted significant influence largely, but not exclusively, through the political parties. The very rich supplied a majority of the participants in each of four sets of major decisions examined and collectively enjoyed power greatly disproportionate to their numbers. Using their money, social prestige, time, access to expertise, political skill, and substantial control of the press, they were able to introduce new ideas, deflect ideas they did not like, limit the ability of politicians to loot the public till, influence nominations, and win office for themselves. Yet the metropolitan region was not dominated by a closed power elite. Since every major decision required government action, universal manhood suffrage and the party organizations that depended on the voters and the organized interest groups did impose limits on the power of the very rich. The power of the very rich was limited, too, by their own large numbers and diversity, by their mutual disagreements, by their need to rely on experts who often held independent views, and by their own political ambitions in the narrowly contested and highly factionalized New York state arena.⁹¹

In Stamford and in Birmingham, Alabama, comparable studies have shown that during the same years the very wealthy, though again not a unified power elite, also enjoyed power greatly disproportionate to their numbers. As in New York, though to a lesser extent, they found it necessary to recognize some

⁸⁹ Feinstein, *Stamford in the Gilded Age*; and Hammack, "Participation in Major Decisions in New York City, 1890-1900."

⁹⁰ These decisions include the 1896 centralization of New York City's public school system; the 1898 consolidation of Brooklyn, Staten Island, Manhattan, and what then became the boroughs of Queens and the Bronx into a single "Greater New York City"; the planning and financing of the first rapid transit subway, completed in 1900; and nominations for mayor of New York City, 1896-1903. For full accounts of the first two sets of decisions and preliminary discussions of the other two, see my "Participation in Major Decisions in New York City, 1890-1900."

⁹¹ Hammack, "Participation in Major Decisions in New York City, 1890-1900," chap. 7. That the analysis-of-decisions method produced these conclusions about the concentration of power in the hands of a small number of economic and political elites is evidence that the method does not, as some have argued, necessarily lead to "pluralistic" findings.

of the preferences of others, particularly those of modest wealth who organized to advance their claims. Between 1800 and 1890, the rapidly increasing size of American cities, the shift off the land, and the dislocations of rapid industrialization may have deprived many of a sense—and perhaps of the reality—of effective participation in community affairs, although this point is by no means established. But if these recent studies of Birmingham, New York, and Stamford are correct, and if we can rely on the studies of New Haven, New York, Chicago, Syracuse, and other cities conducted by political scientists during the 1950s and 1960s,⁹² power—as measured by successful participation in community decisions—has become distributed in an increasingly dispersed fashion since the end of the nineteenth century. This is not to say that power is now equally distributed; the effect of this shift on the power of the poorer income classes is disputed and unclear. The very wealthy have not withdrawn from public affairs, but they have increasingly found themselves divided over community issues and forced to bargain with other elites. Future research might profitably inquire into the material, social, institutional, and mental conditions that, in the nineteenth century and increasingly in the twentieth, have divided the rich and encouraged others to organize.

During the twentieth century, New York City, the one community for which we now have comparable studies for the 1890s and the 1950s, changed in a great many ways that may have encouraged a more pluralistic distribution of power: its population grew, then stabilized in numbers, while becoming more diverse ethnically; its population greatly increased its mastery of the English language and its absolute standard of living; its political and legal climate became more hospitable to labor unions; it retained its great number and diversity of small businesses while becoming, through the first half of the century, more and more the headquarters for national corporations; it improved its already sophisticated communications system. New York City's government gave up a ward-based council, but retained a strong mayor; its political party organizations remained vigorous. Local, state, and national agencies took over more and more activities, many of which require great technical competence. Expertise became much more generally available to government agencies and to nonelite interest groups.⁹³ Many of these changes have taken place in other American communities, often somewhat later than they did in New York. But we will not be able to evaluate their relative influence, or the influence of other factors, until other studies permit us to compare the distribution of power in American communities of all sorts, in all regions, since 1800.

⁹² Dahl, *Who Governs? Democracy and Power in an American City*; Raymond E. Wolfinger, *The Politics of Progress* (Englewood Cliffs, N.J., 1974); Sayre and Kaufman, *Governing New York City*; Lowi, *At the Pleasure of the Mayor: Patronage and Power in New York City*; Edward C. Banfield, *Political Influence* (Glencoe, Ill., 1961); Roscoe C. Martin et al., *Decisions in Syracuse* (Bloomington, Ind., 1961); and Linton C. Freeman, *Patterns of Local Community Leadership* (Indianapolis, Ind., 1968). For other works, see the useful annotated listing in Willis D. Hawley and James H. Svara, *The Study of Community Power: A Bibliographic Review* (Santa Barbara, Calif., 1972).

⁹³ Sayre and Kaufman have noted several of these changes in New York; *Governing New York City*, 18–36. Other changes emerge in a comparison of the decision-making process at the end of the nineteenth century with the process described by Sayre and Kaufman.

The Metaphysics of Law: An Essay on the Very Young Marx

DONALD R. KELLEY

“IN A WORD I HATE ALL THE GODS!” Karl Marx took this defiant cry of Prometheus, “the eminent saint and martyr of the philosophical calendar,” as the motto and point of departure of his doctoral thesis presented at the University of Jena in 1841, and in many ways it was symbolic of his entire career.¹ As a student at the Gymnasium of Trier in 1835, the seventeen-year-old Marx, still a practicing Lutheran, was already celebrating the power of man to determine his own fate and envisaging the Promethean task of re-creating the world by uncovering its first principle, its *arche*, and using it for the benefit of all mankind. “History calls those men greatest,” he wrote in his reflections on the choice of a profession, “who have ennobled themselves by working for the common good”; and this heaven-, or rather earth-, storming goal Marx also preserved for the rest of his life.²

Too little attention has been paid to the profession first chosen by Marx, namely, that of the law. For centuries the law had been represented by its practitioners as the most direct path to the “common good” (*bonum publicum*). In general (and intellectually genetic) terms it was by pursuing jurisprudence—not always the substance and methods, perhaps, but certainly the ideals of jurisprudence—that Marx began the heroic task by which he hoped to attain greatness. Needless to say, his enterprise was not without setbacks, and of these one of the first was Marx’s disillusionment with conventional jurisprudence as it actually related to political and social matters. Another was the resistance of political authority, and with it professional law, to the pursuit of the social ideals embodied in jurisprudence. This resistance Marx felt most directly in the censorship legislation directly aimed at the *Rheinische Zeitung*, on which he served his apprenticeship in practical politics in the two

My thanks for the proddings and suggestions of friends and colleagues at the Institute for Advanced Study, who heard one version of this paper, and of those at the University of Rochester (especially Peter Linebaugh), who have shown forbearance toward still another perspective on Marxism.

¹ Marx, “Dissertation und Vorarbeiten über die Differenz der demokratischen und epikurischen Naturphilosophie,” in *Marx-Engels Gesamtausgabe* (Berlin, 1927–32), 1, pt. 1: 10 (translations throughout from *Marx-Engels Collected Works* [New York, 1975–], 1: 30). Also see G. Cottier, *L’Athéisme du jeune Marx* (Paris, 1959).

² Marx, “Betrachtung eines Jünglings bei der Wahl eines Berufes,” in *Marx-Engels Gesamtausgabe*, 1, pt. 2: 167 (*Collected Works*, 1: 8).

years after receiving his doctor's degree. In 1843 Marx resigned in protest, and, interestingly enough, his statement was accompanied by a representation of Prometheus with his entrails being devoured.³ Yet Marx's own Promethean enterprise continued, and it may not be too fanciful to see his system of political economy as the final metamorphosis of his first calling.

Marx's commitment to a legal career began in the fall of 1835, when he entered upon the study of civil law at the University of Bonn. Even then, he showed distressing signs of unorthodoxy, and his father worried about his romantic notions. "In connection with the lectures on law," Heinrich Marx wrote in November 1835, "you must not demand [that they] . . . should be touching and poetic"; and he continuously urged his son to be more practical-minded.⁴ In a stream of letters father taxed son with a variety of time-honored adolescent failings that seemed to reflect idealism and rebelliousness. Most prominent among these were disorder ("One expects *order* even from a scholar, and especially from a practical lawyer"), financial irresponsibility (" . . . and you know quite well I am not rich"), selfishness (" . . . in your heart egoism is predominant"), and a dangerous tendency to violence⁵ ("Is duelling then so closely interwoven with philosophy?"). Karl had indeed been charged with "rowdiness and drunkenness at night." Despite such compulsive "sermonizing," the elder Marx tried not to insist on his authority, but the "wild goings-on at Bonn" made it clear that his son was willful and defiant and had set his sights on a goal higher than a conventional legal or cameralistic career. Academically, however, Marx was declared "diligent" and even "very diligent and attentive" (*sehr fleissig und aufmerksam*) in all ten of the courses he took during 1835–36, six of which dealt with branches of law and legal history.⁶

His transfer to the University of Berlin in the fall of 1836 marked a turning point in Marx's life and very noticeably strengthened his attachment to legal studies. Although its most eminent philosopher, Georg Wilhelm Friedrich Hegel, had been dead for five years, the university was still among the most impressive in Europe, and its faculty of law in particular was a center of intellectual ferment. The most eminent professor was Friedrich Karl von Savigny, one of the greatest legal scholars of the century and regarded as leader of the intimidating "historical school of law," which emerged in opposition both to natural law theories and to rationalistic efforts at codification, especially that of Napoleon. The second most renowned professor was the Hegelian Eduard Gans—"a versatile Jew," sniffed Heinrich von Treitschke, "acute rather than intelligent"—who had emerged as a leading critic of this same historical school. The tendency of the historical school was, as Sidney Hook has put it, "to nail the present upon the cross of the past";

³ *Marx-Engels Gesamtausgabe*, 1, pt. 1: frontispiece (*Collected Works*, 1: 374).

⁴ Heinrich Marx to Marx, Nov. 18–19, 1835, in *Marx-Engels Gesamtausgabe*, 1, pt. 2: 186 (*Collected Works*, 1: 646). Also see the forthcoming biography by Jerrold Seigel.

⁵ Heinrich Marx to Marx, early 1836, Mar. 19, 1836, May–June 1836, Dec. 9, 1836, in *Marx-Engels Gesamtausgabe*, 1, pt. 2: 188, 191, 192, 226 (*Collected Works*, 1: 650, 652, 653, 689).

⁶ "Abgangszeugnis der Universität Bonn für Marx," Aug. 22, 1836, in *Marx-Engels Gesamtausgabe*, 1, pt. 2: 194 (*Collected Works*, 1: 658). On Marx's legal studies, see August Cornu, *Karl Marx et Friedrich Engels*, 1 (Paris, 1955): 81ff.

and Savigny himself had to defend his school against the charge that it proposed to surrender to the “tyranny of the past” (*die Herrschaft der Vergangenheit*).⁷ In contemporary debates Savigny and Gans were at odds over the reform of Prussian law as well as the academic problem of “possession” in civil law. One concerned French scholar, who in 1827 had published a thesis on Savigny, observed, “The war is spectacular. In the historical school they are afraid of philosophy. . . . In the philosophical camp they look down with pity on purely historical jurists”⁸ In 1839 this war between the “realism” of Savigny and the “idealism” of Gans became public. At least three years earlier, however, Marx had already been exposed to this juridical and philosophical *Kulturkampf*. In his first year at the university he studied “Pandekten” with Savigny and “Kriminalrecht” with Gans, and there is little doubt that their altercation had a shaping effect on Marx’s own developing social views.⁹

IF THE PERSPECTIVE HERE SEEMS ECCENTRIC, it is nevertheless precisely the one that the young Marx presented in the one autobiographical statement of his prerevolutionary years. This is the famous confessional letter which he wrote to his father at the end of his first year at the University of Berlin (November 10, 1837). “There are moments in one’s life,” he began, “which are like the frontier posts marking the completion of a period but [which] at the same time clearly indicate a new direction.”¹⁰ Looking back, he saw the beginning of a “new life” linked with his love for Jenny von Westphalen and expressed in his efforts to write lyrical poetry. During that same year, in fact, he wrote two “wild songs” on “nocturnal love” and a Faustian “fiddler,” reflecting some of his adolescent pangs and “striving for poetic fire.” But, like Dante Alighieri, whose “new life” was also associated with passion and poetry, Marx was impelled to turn from love to learning—from the “remote heaven” of his literary art, as he put it, to more adult aims. “Poetry, however, could be and had to be only an accompaniment,” he continued in the letter to his father; “I had to study law and above all felt the urge to wrestle with philosophy.”

What is essential to understand at this point is that Marx did not distinguish at all sharply between these two disciplines, for the identification of law and philosophy was among the first lessons learned by law students. “Jurisprudence is the true philosophy” is a formula taken from the very first

⁷ Hook, *From Hegel to Marx* (Ann Arbor, 1950), 142. In general, see Adolph Stoll, *Friedrich Karl von Savigny*, 2 (Berlin, 1929): 184–88; Hans Günther Reissner, *Eduard Gans: Ein Leben in Vormärz* (Tübingen, 1965), 123, 159; and Hermann Kantorowicz, “Savigny and the Historical School,” *Law Quarterly Review*, 53 (1937): 326–43.

⁸ E. Lerminier, *Introduction générale à l’histoire du droit* (Paris, 1829), 270, and *De Possessione Analytica Savigniana Doctrinae Exposito* (Paris, 1827). See Max Lenz, *Geschichte der königlichen Friedrich-Wilhelms-Universität zu Berlin*, 2 (Halle, 1910): 384.

⁹ “Abgangszeugnis der Universität für Marx,” in *Marx-Engels Gesamtausgabe*, 1, pt. 2: 247 (*Collected Works*, 1: 703). According to the contemporary student catalogue, Marx must have taken “Privatim ius Pandectarum” from nine to eleven every morning with Savigny and “Ius criminale universale” from noon to one every afternoon except Wednesday with Gans as well as a third course on “anthropologia” with H. Steffens. See *Index lectionum . . . in Universitate Friderica Guilelma* (Berlin, 1837).

¹⁰ Marx to Heinrich Marx, in *Marx-Engels Gesamtausgabe*, 1, pt. 2: 213–31 (*Collected Works*, 1: 10–21); and “Nachtliebe” and “Spielmann” in *Marx-Engels Gesamtausgabe*, 1, pt. 1: 147–48 (*Collected Works*, 1: 22–24).

sentence of the Pandects, that seminal collection of classical jurisprudence which Marx had not only studied (with Savigny the previous year) but had even begun to translate into German.¹¹ The justification for this identification had been two-fold: first, that law constituted a true “science” (*civilis scientia*), since it was universal and viewed the world in terms of cause and effect, and, second, that unlike natural science it proposed as its principal object the common good of society. From the thirteenth century to Marx’s own time the theme *jurisprudentia est vera philosophia* was pursued by jurists, commentators, and systematizers of law, including contributors to the French Civil Code of 1804. Whatever the relations between mature Marxism and classical “civil science,” the young Marx, at least the very young Marx, started in the old tradition. “The two,” he wrote of law and philosophy, “were so closely linked that, on the one hand, I read through Heineccius, Thibaut and the sources quite uncritically, in a mere schoolboy fashion; thus, for instance, I translated the first two books of the Pandect[s] into German, and, on the other hand, tried to elaborate a philosophy of law covering the whole field of law.”¹²

In these youthful experiments Marx carried out academic assignments at the same time he was also carrying on the great tradition of systematic jurisprudence which grew out of the work of the scholastic jurists (surveyed in Savigny’s classic history of Roman law in the Middle Ages) and which gained maturity in the “reformed jurisprudence” established in the sixteenth century, most prominently at the University of Bourges. From here, especially through the efforts of Hugues Doneau (after his transferral to Heidelberg and, later, to Altdorf), the values and program of this philosophical school made their way into German academic jurisprudence; and the tradition was preserved and extended by generations of “Systematiker,” as Savigny called them, down to the nineteenth century.¹³ Among these were two distinguished

¹¹ For the general orientation of this paper, see my “Vera Philosophia: The Philosophical Significance of Renaissance Jurisprudence,” *Journal of the History of Philosophy*, 14 (1976): 67–79, “Louis le Caron, Philosophe,” in *Philosophy and Humanism: Renaissance Essays in Honor of Paul Oskar Kristeller*, ed. E. Mahoney (New York, 1976), 16–35, and “Vico’s Road,” in *Giambattista Vico’s Science of Humanity*, ed. G. Tagliacozzo and D. Verene (Baltimore, 1976), 15–29.

¹² Marx to Heinrich Marx, Nov. 10, 1837, in *Marx-Engels Gesamtausgabe*, 1, pt. 2: 215: “Nun durfte und sollte die Poesie nur Begleitung sein; ich musste Jurisprudenz studieren und fühlte vor allem Drang, mit der Philosophie zu ringen. Beides wurde so verbunden, dass ich teils Heineccius, Thibaut und die Quellen rein unkritisch, nur schülerhaft durchnahm, so z. B. die zwei ersten Pandektenbücher ins Deutsche übersetzte, teils eine Rechtsphilosophie durch das Gebiet des Rechts durchzuführen suchte. Als Einleitung schickte ich einige metaphysische Sätze voran und führte dieses unglückliche Opus bis zum öffentlichen Rechte, eine Arbeit von beinahe dreihundert Bogen. Vor allem trat hier derselbe Gegensatz des Wirklichen und Sollenden, der dem Idealismus eigen, sehr störend hervor und war die Mutter folgender unbehilflich unrichtiger Einteilung. Zuerst kam die von mir gnädig so getaufte Metaphysik des Rechts, d. h. Grundsätze, Reflexionen, Begriffsbestimmungen, getrennt von allem wirklichen Rechte und jeder wirklichen Form des Rechts; wie es bei Fichte vorkommt, nur bei mir moderner und gehaltloser. Dabei war die unwissenschaftliche Form des mathematischen Dogmatismus, wo das Subjekt an der Sache umherläuft, hin und her rasonniert, ohne dass die Sache selbst als reich Entfaltendes, Lebendiges sich gestaltete, von vornherein Hindernis, das Wahre zu begreifen.”

¹³ In this wilderness of juridical literature, almost wholly uncharted by historians except through the Hegelian connection, these are just a few of the most relevant works: the various systematic efforts of the Enlightenment jurist and historian Heineccius, notably the *Elementa Juris Naturae et Gentium* and the two volumes of *Elementa Juris Civilis*, arranged respectively according to the order of the Pandects and the Institutes of Justinian, in many editions and translations (including one by Thibaut); Gustav Hugo, *Institutionen des römischen Rechts*; Thibaut, *System des Pandekten-Rechts*; Gans, *System des römischen Civilrechts*;

names on Marx's reading list, Johann Gottlieb Heineccius and A. F. J. Thibaut, and his teachers at Berlin, including A. F. Rudorff as well as Savigny and Gans. All of these scholars followed variations of the old scholastic method associated with the fourteenth-century jurist Bartolus, a method based upon civil law principles that had been correlated and rationalized by generations of "authorities" working to achieve greater consistency, generality, and equity. In particular, they directed their efforts to the creation of an intelligible (and teachable) scheme of law based upon classical principles or organization, especially the Pandects.

So, evidently, did Marx. The three-hundred-page "unhappy work" referred to in his letter has not survived, but Marx did set down a brief outline for his father's edification. He began by making the conventional civilian distinction between private and public law, dividing the former into the equally conventional rubrics of the law of persons, things, and persons in relation to property, with special emphasis on the law of contracts. In its ramifying and trichotomizing (*trichotomische*) approach, Marx's scheme could not have differed substantially from innumerable such schemes devised by students of law over the centuries, especially in connection with eighteenth-century movements for codification.¹⁴ What was unconventional was Marx's reaction to his own work, and he laid bare its deficiencies in his usual intemperate language. "The whole thing is replete with tripartite divisions, it is written with tedious prolixity, and the Roman constitutions are misused in the most barbaric fashion in order to force them into my system." Worst of all, it embodied the basic contradiction of the law of the old regime. Significantly, when Marx reached the public law section of his treatise, that is, the point at which civil rules joined up with political goals and questions of the common good, he "saw the falsity of the whole thing" and broke off the project.

The source of the error lay with what Marx was pleased to call "the metaphysics of law," which he defined as "basic principles, reflections, definitions of concepts, divorced from all actual law and every actual form of law." According to Marx, the "metaphysics of law" was akin to Kantian idealism and to allegedly "scientific" systems of natural law, such as that constructed by Johann Fichte, but it was even worse because of the way it manipulated social reality. The difficulty stemmed from the time-honored "opposition between what is and what ought to be," which in turn was "the source of the hopelessly incorrect division of the subject matter." "Mathematical dogma-

Savigny, *System des heutigen römischen Rechts*; as well as other works referred to by Marx. See note 18, below. In general, see Paul Koschaker, *Europa und das römische Recht* (Berlin, 1953); E. Landsberg, *Geschichte der deutschen Rechtswissenschaft*, 3 (Berlin, 1910); Franz Wieacker, *Privatrechtsgeschichte der Neuzeit* (Göttingen, 1967); and Guido Fassò, *Storia della filosofia del diritto*, vols. 2, 3 (Bologna, 1968-70).

¹⁴ See A.-J. Arnaud, *Les Origines doctrinales du Code civil français* (Paris, 1969); and Klaus Luig, "Institutionen-Lehrbücher der nationalen Rechts im 17. und 18. Jahrhundert," *Ius Commune*, 3 (1970): 64-97. Marx's own contribution to this tradition followed the Roman form of Justinian and Gaius, which likewise distinguished the *ius publicum* from the *ius privatum* and divided the latter into the laws of persons, things, and actions (or, in Marx's terms for this last, persons in relation to property). In this Marxian trichotomy, the first two rubrics—persons (the basis of "subjective" law) and things (the objects in the natural world that are potentially property)—correspond in a general way to the Hegelian dialectic of subject-object but in a sequence that Marx rejected. See page 365, below.

tism” might be appropriate for the abstractions of geometry, which are bound neither by social context nor by historical change. “On the other hand,” Marx explained, “in the concrete expression of a living world of ideas, as exemplified by law, the state, nature, and philosophy as a whole, the object itself must be studied in its development; arbitrary divisions must not be introduced, the rational character of the object itself must develop as something imbued with contradictions in itself and find unity in itself.”¹⁵ It seems significant that this, the first Marxian prefiguring of historical materialism, should be formulated in the context of a fundamental, and even revolutionary, critique of systematic jurisprudence. The subversive consequences were not lost on Marx’s father, and his warning was prophetic. “Your views on law are not without truth,” he wrote in response to his son’s letter, “but are very likely to arouse storms if made into a system, and are you not aware how violent storms are among the learned?”¹⁶

Marx’s break-through was certainly an act of scholarly dissent, even heresy, since he was defying authorities honored as “priests of the laws” (*sacerdotes legum*) in the Pandects and, hence, in legal tradition. But it was also a moral epiphany attended by a deep personal crisis. Disillusionment with the law sent him back to pure philosophy “with a good conscience,” as he put it, and even to poetry; and these obsessions in turn so undermined his health that he was obliged to go to the country for a rest. The trip seemed to be effective. He returned not only with renewed strength but also with a resolve to turn away from youthful transcendentalism back to the substance of life. “I even joined my landlord in a hunting excursion,” he wrote, “rushed off to Berlin and wanted to embrace every street-corner loafer.” He returned with renewed interest in Hegel, whose works he read through for the first time in order “to bring genuine pearls into the light of day”; and this study coincided with his attendance at meetings of the radical Hegelian Doctors’ Club. Such was the emotional context of Marx’s initial revulsion from idealism. “A curtain has fallen, my holy of holies was rent asunder, and new gods had to be installed” is the way he expressed his traumatic experiences. “From the idealism which, by the way, I had compared and nourished with the idealism of Kant and Fichte, I arrived at the point of seeking the idea in reality itself. If previously the gods had dwelt above the earth, now they became its centre.”¹⁷ From this

¹⁵ Marx to Heinrich Marx, in *Marx-Engels Gesamtausgabe*, 1, pt. 2: 215: “Das Dreieck lässt den Mathematiker konstruieren und beweisen, es bleibt blosse Vorstellung im Raume, es entwickelt sich zu nichts Weiterem, man muss es neben anderes bringen, dann nimmt es andere Stellungen ein, und dieses verschieden an dasselbe Gebrachte gibt ihm verschiedene Verhältnisse und Wahrheiten. Dagegen im konkreten Ausdruck lebendiger Gedankenwelt, wie es das Recht, der Staat, die Natur, die ganze Philosophie ist, hier muss das Objekt selbst in seiner Entwicklung belauscht, willkürliche Einteilungen dürfen nicht hineingetragen, die Vernunft des Dinges selbst muss als in sich Widerstreitendes fortrollen und in sich seine Einheit finden.”

¹⁶ Heinrich Marx to Marx, Dec. 28, 1836, in *Marx-Engels Gesamtausgabe*, 1, pt. 2: 199 (*Collected Works*, 1: 665).

¹⁷ Marx to Heinrich Marx, in *Marx-Engels Gesamtausgabe*, 1, pt. 1: 218: “Ein Vorhang war gefallen, mein Allerheiligstes zerrissen, und es musste neue Götter hineingesetzt werden. Von dem Idealismus, den ich, beiläufig gesagt, mit Kantischem und Fichteschem verglichen und genährt, geriet ich dazu, in Wirklichen selbst die Idee zu suchen. Hatten die Götter früher über die Erde gewohnt, so waren sie jetzt das Zentrum derselben geworden.”

position it was not far to a Promethean—and materialistic—denial of the “gods” altogether.

In this way, awakened as it were from his dogmatic slumber, Marx turned from the “ideal in itself” (*Begriff an sich*) to the “thing in itself” (*Ding an sich*), to use a Kantian expression that would not have pleased him. Yet he continued to think in juridical terms and, indeed, to apply to juridical sources for enlightenment, though in a very different style from the “metaphysics of law.” “Shortly after that,” he wrote, “I pursued only positive studies”; and he listed a variety of works on civil law by Savigny and others as well as Gratian’s twelfth-century collection of canon law and some studies in early Germanic law.¹⁸ Marx rejected the error, which he “shared with Herr v. Savigny,” that separated legal form from content—that is, legal concepts from “positive” or “material” law. Instead, he believed that “positive law in its conceptual development” was exactly the same as “the formation of the concept of law,” and so he concluded that philosophical understanding had to proceed, above all, through the *jus positivum*, since this law was the meeting place between legal norms and social reality. To fulfill the ideal of jurisprudence as true philosophy, in other words, one had to investigate first the material base. This train of thought quite likely led Marx not only to reject the “metaphysics of law” but also to turn to another discipline entirely as the principal vehicle of his social philosophy.

This interpretation of the very young Marx is made more plausible by examining the particular work in which he discovered the basic fallacy of the “metaphysics of law”: the first book of his “positive studies,” Savigny’s great treatise on possession, *Das Recht des Besitzes*, which one authority regards as the most influential legal monograph of modern times.¹⁹ In this work Savigny exhibited the same enormous range of learning as in his monumental history of Roman law, but in *Das Recht des Besitzes* he applied it in a practical and even polemical fashion. He discussed the whole complex of problems involved in possession and property with reference to the judgments of ancient authorities, especially those included in the Pandects, and of later commentators. The latter he divided into two classes: the “interpreters,” including medieval and Renaissance jurists such as Portius Azo, Accursius the glossator, Andrea Alciato, and Jacques Cujas, as well as such modern authors as his colleagues Gans and Rudorff; and the “systematists,” including Hugues Doneau, Doneau’s mentor François le Douaren, R.-J. Pothier, and Savigny’s rival A. F. J. Thibaut. In scholastic fashion he listed, criticized, and usually rejected the *opinionones* of these and many other jurists before presenting his own interpretation. In later editions the collecting of opinions and controversies continued (over fifty were treated in the seventh edition, published by Rudorff).²⁰

¹⁸ Marx mentions Savigny, *System des heutigen römischen Rechts*; Paul Feuerbach, *Lehrbuch des gemeinen in Deutschland gültigen peinlichen Rechts*; C. L. W. von Grolmann, *Grundsätze der Criminalrechtswissenschaft*; A. W. Cramer, *De Verborum Significatione*; J. N. von Wening-Ingenheim, *Lehrbuch des Gemeines Civilrechts*; C. F. Mühlenbruch, *Doctrina Pandectarum*; W. A. Lauterbach, certain titles on civil process (“einzelne Titel . . . Zivilprozess”); Gratian, *Concordia discordantium canonum*; and G. P. Lancellotti, *Institutiones Juris Canonici*.

¹⁹ Eugen Ehrlich, *Fundamental Principles of the Sociology of Law*, trans. W. Moll (New York, 1962), 320.

²⁰ Savigny, *Das Recht des Besitzes* (7th ed.; Berlin, 1865), 10, 543. Also see Hasso Jaeger, “Marx et Savigny,” *Archives de philosophie du droit*, 12 (1967): 65–89 (in a volume devoted generally to “Marx et le droit

The entire exercise was based on the assumption—rejected, of course, by Marx—that legal form, by which Savigny meant classification in the Roman system, could be distinguished from content, by which he meant specific formulation by Roman and modern jurists.

Even to the young Marx this book must have seemed a monstrosity of antiquarian learning without any reasonable relationship to the common good. At the outset the reader is instructed that the subject of the treatise is not the right to possess (*jus possidendi*), but rather the rights arising from simple possession (*jus possessionis*); and these rights Savigny grouped under two headings, usucapion and interdiction.²¹ Usucapion signified uncontested possession (in the law of the Twelve Tables for one or two years), and it was related to the legal concept of prescription (the *praescriptio longae temporis* of Roman and canon law). Interdiction referred to the right to repossess something that had been seized by act of violence and so arose from redress of injury. Savigny also distinguished between “natural” and “civil” possession, that is, physical possession in the state of nature and the juridical condition that created “property,” which was generally defined as “the totality of all real rights.” Although he employed such legal concepts as *bona fides* and *justa causa*, Savigny’s argument seemed to be based ultimately upon the accidents of Roman history, or prehistory, in which private property (such as the *ager privatus* discussed by the great Roman historian Niebuhr) arose out of the simple act of prehension, or willful seizure, and then somehow was given legal status by receiving customary acceptance and general social formulation. Out of *factum*, in juridical terms, came *jus*. Might makes right—perhaps even property is theft, if one can so render the euphemism “prehension”—so it may have seemed to a young and idealistic reader like Marx.

On this foundation, in any case, Savigny erected his theory of possession, and hence of property; and in reaction to this Gans launched an assault on the historical school of law in his own discussion of the problem of possession. This pamphlet, published in 1839, was an exercise in “scientific polemic” in which Gans bitterly attacked Savigny’s sophistical habit of transforming hypotheses into demonstrated theses with the help of strategically placed “thuses” and “in facts.” All Savigny really did was argue from Roman history, Gans charged; and for all their “practical sagacity” the Romans had no national claims to philosophical truth. “Possession is a fact, a natural condition and not a right”—so Gans stated Savigny’s position—“but nonetheless the possessor has rights because of his possession.” Such empiricist (and historicist) logic Gans totally rejected. When Savigny defined possession in terms of interdiction and usucapion, Gans complained, “he removed the subject to a field in which one cannot contend with philosophical weapons: as soon as pure theory is mentioned, he confuses the matter with terms which theory has

moderne”); Georges Cornil, “Philosophie et droit privé,” *Revue de droit international et de législation comparée*, 2d ser., 10 (1909): 616–44; and Peter Landau, “Karl Marx und die Rechtsgeschichte,” *Tijdschrift voor Rechtsgeschiedenis*, 41 (1973): 361–71.

²¹ Savigny, *Das Recht des Besitzes*, 43.

nothing to say about”²² It was obviously no way to go about realizing the ideal of jurisprudence as true philosophy.

Marx had no trouble choosing sides in this debate. Even if he had not shared Gans’s Hegelian bias at this point, he would surely have been repelled by the method and, by inference, the morality of his other mentor, Savigny. In the first place, Marx had come to reject the idea of understanding and, still less, of legitimizing social institutions within the framework of an arbitrary system of civil law. Secondly, because of his concern for social justice he could have had little sympathy with Savigny’s tendency to justify the entire range of property rights on the basis of the fact of possession, which is to say on the basis of an amoral and irrational state of nature. Clearly, it was a theory at odds with the view that Marx later came to adopt toward property, especially in relation to labor. For these reasons, it may be suggested that Marx’s first act of academic subversion was to turn Savigny, not Hegel, on his head.

THIS INTELLECTUAL TRAUMA occurred in Marx’s twentieth year, when he was still contemplating a legal career—in jurisprudence rather than cameralistics. In the next few years several things deflected him from this course. One was his immersion in traditional philosophy, not only that of ancient Greece for purposes of his thesis on Democritean and Epicurean philosophy, but also (and even more intensively) Hegelianism, which was situated at the vortex of many burning social as well as conceptual problems of the day. Another was the death of his father, whose moral pressure and financial support were both withdrawn in 1838. Finally, there was the official condemnation of the Left Hegelians, which blocked Marx from an academic career. In 1842 he began his career as a political journalist, first submitting articles to and then becoming chief editor of the *Rheinische Zeitung*. At last, he had occasion to contribute directly to the “common good” by commenting upon a variety of social issues, including freedom of the press and criminal law. Yet Marx still preserved some of his juridical-mindedness. He criticized the Prussian censorship legislation of 1841 on the grounds not merely of its injustice but also of its illegality. Again, he took up the theme of the contradictions between law and its professed ideals. “Is *truth* to be understood as being simply *what the government decrees* . . . ?”²³ Having overcome academic authority, Marx was setting his sights on more intimidating obstacles to the common good.

Marx kept coming back to the issues posed by his old teacher Savigny, who

²² Gans, *Ueber die Grundlage des Besitzes, Eine Duplik* (Berlin, 1839), 7, 11. Gans had offered a similar, though milder, criticism of Savigny’s famous *Geschichte des römischen Rechts im Mittelalter* in a review complaining that the work was merely literary history without conceptual value and concluding that “einer innerliche Geschichte” remained to be written; see his *Vermischte Schriften* (Berlin, 1834), 1: 46. For other entries in the contemporary debate over possession, see the list (not exhaustive) by A. Rudorff in his appendix to Savigny, *Das Recht des Besitzes*, 543–62. During the years 1836–42 there were over forty publications, not including popular discussions of the problem of property such as that of Pierre Proudhon; and throughout the century the literature continued to accumulate. For a modern discussion, see Gunter Wesener, “Ius Possessionis,” *Festschrift für Max Kaser* (Munich, 1976), 715–42.

²³ Marx, “Bemerkungen über die neueste preussische Zensurinstruktion,” in *Marx-Engels Gesamtausgabe*, 1, pt. 1: 154 (*Collected Works*, 1: 113).

in 1842 was appointed Prussian minister of justice for the reform of laws. "Are not," he asked, "most of your court cases and most of your civil laws concerned with property?"²⁴ A case in point was a recent statute concerning the theft of wood, which in effect had raised forest regulations to the level of criminal law. Even the penal code of the sixteenth century, Marx noted, had not gone so far in the protection of property. What he objected to was precisely the sort of proprietary claims argued by Savigny—"customs contrary to law," as Marx put it, and benefitting only the privileged, landowning classes. "Their origin," he added, "dates to the period in which human history was part of *natural history*," that is, the "spiritual animal kingdom" of feudalism. Marx celebrated universal human law above such class privilege. "We demand for the poor a customary right, and indeed one which is not of a local character but is a customary right of the poor in all countries."²⁵ In this article Marx thought he had uncovered one of the basic flaws of modern "liberal legislation," which was "formulating and raising to a universal level those rights which they found already in existence," so raising *de facto* conditions to a *de jure* level. This is what came of arguing from the selfish individualism of a pre- and anti-social state of nature, instead of from the rational ideal of "the state as the great organism, in which legal, moral, and political freedom must be realized . . ."²⁶

Turning his journalistic attention to such practical problems, Marx began to formulate more clearly his views about the relationship between law and social reality—a problem that was congruent in many ways with that of the relationship between ideology and society's material base. These views have almost invariably been interpreted in terms of Marx's Hegelianism, and with justice. Yet it is clear that he was still involved in academic jurisprudence, for he devoted to that subject the most comprehensive and aggressive statement of his philosophic position up to that time: his pivotal and, as Sidney Hook has remarked, "strangely neglected" critique of the historical school of law. This school, whose manifesto was Savigny's celebrated essay on "the vocation of our age for legislation and jurisprudence," set itself against the optimistic and rationalistic philosophy of law which had prevailed in the previous century.²⁷ In his counterattack, "The Philosophical Manifesto of the Historical School of Law," Marx not only announced his abjuration of the profession of law but also suggested the premises for a more positive and critical basis for social thought and action.²⁸ In part, his attack was a typical act of academic

²⁴ Marx, "Der leitende Artikel in Nr. 179 der Kölnischen Zeitung," in *Marx-Engels Gesamtausgabe*, 1, pt. 1: 246 (*Collected Works*, 1: 199).

²⁵ Marx, "Debatten über das Holzdiebstahlgesetz," in *Marx-Engels Gesamtausgabe*, 1, pt. 1: 272 (*Collected Works*, 1: 230). Also see C. Vigouroux, "Karl Marx et la législation forestière rhénane de 1842," *Revue d'histoire économique et sociale*, 43 (1965): 222–33.

²⁶ Marx, "Der leitende Artikel," in *Marx-Engels Gesamtausgabe*, 1, pt. 1: 249 (*Collected Works*, 1: 202).

²⁷ Savigny, *Vom Beruf unserer Zeit für Gesetzgebung und Rechtswissenschaft* (Heidelberg, 1814), reproduced with other material relating to the quarrel with Thibaut in J. Stern, *Thibaut und Savigny* (Darmstadt, 1959).

²⁸ Marx, "Das philosophische Manifest der historischen Rechtsschule," in *Marx-Engels Gesamtausgabe*, 1, pt. 1: 251–59 (*Collected Works*, 1: 203–10). For a recent discussion, though largely in the Hegelian context, see Christoph Schefold, *Die Rechtsphilosophie des jungen Marx von 1842* (Munich, 1970). For the lexicon of Marx's juridical vocabulary and a historical commentary, see R. Guastini in *Materiali per una storia della cultura giuridica*, ed. G. Tarello, 3 (Genoa, 1972): 349–515.

rebellion, although he disguised his doctrinal parricide somewhat by taking as his ostensible target not *Herr Professor* and now *Herr Minister* Savigny (referred to only as “the most famous historical jurist”) but rather Savigny’s elder colleague Gustav Hugo, whom Marx called the “forefather” (*Ältervater*) and “original type” (*Naturmensch*) of the historical school of law. In any case, this declaration of independence was the first step in the enterprise of philosophical criticism that cleared the ground for Marx’s own system of thought.

Like Savigny, Hugo affected a “reaction against the frivolous spirit of the eighteenth century”; but, in fact, Marx charged, it was the historical school that was frivolous and, worse still, irrational and immoral. In a sense the basic flaw was that of professional jurisprudence, that is, the eclectic, indiscriminate, and uncritical use of sources. Marx’s complaints about Hugo (he “knows no distinctions”) could have been applied as well to Savigny’s treatise on possession. “Everything existing serves him as an authority, every authority serves him as an argument.” Hugo’s position was a travesty of Kant’s and had been arrived at “by supposing that, because we cannot know what is true, we consequently allow the untrue . . . to pass as fully valid.” Although insisting that the positive was irrational (“positive, i.e., uncritical” was Marx’s gloss), Hugo held, nevertheless, that it, not reason, supplied the standard of human judgment, since in a sense it embodied natural law, undeniably and, indeed, tautologically reflecting the “natural” progression of events. This, Marx concluded, “is the frank, naive, reckless method of the historical school Hugo, therefore, profanes all that the just, moral political man regards as holy, but he smashes these things only to be able to honour them as historical relics; he desecrates them in the eyes of reason in order afterwards to make them honourable in the eyes of history and at the same time to make the eyes of the historical school honourable.”²⁹

What the historical school of law had accomplished, it would seem, was to make history self-justifying, to derive right from fact, as Savigny had done for property; and the result was the betrayal not only of reason but also of man himself. Absolute skepticism, absolute immoralism, and absolute relativism (if this is not a contradiction) were the foundations of the “positive” philosophy according to Hugo. “The sole juridical feature of man is his animal nature” was the proposition which Marx took to be the motto of the historical school and in particular of Hugo’s *Natural Law as a Philosophy of Positive Law*, to which Marx’s article was ostensibly devoted.³⁰ Among the consequences of Hugo’s attitude was that slavery could be at least “provisionally lawful” and old regime jurists had indeed justified this argument for centuries, largely on the basis of the “law of nations” (*jus gentium*), which was a repository for much of the indiscriminate and immoral “positive law” of the historical school.

²⁹ Marx, “Das philosophische Manifest,” in *Marx-Engels Gesamtausgabe*, 1, pt. 1: 252, 254 (*Collected Works*, 1: 206, 209).

³⁰ Hugo, *Lehrbuch des Naturrechts als einer Philosophie des positiven Rechts* (Berlin, 1809), 43; Hugo discussed “Der Mensch als Thier” in an introductory chapter on “Juristische Anthropologie.” Also see Arno Buschmann, *Ursprung und Grundlagen der geschichtlichen Rechtswissenschaft, Untersuchungen, und Interpretationen zur Rechtslehre Gustav Hugos* (Krefeld, 1963); and, most recently, Jürgen Blühdorn, “Naturrechtskritik und Philosophie des positiven Rechts,” *Tijdschrift voor Rechtsgeschiedenis*, 45 (1974): 3–17.

What is more, Hugo argued, “slavery” was better than a state of war and “not much worse” than poverty, since the “slave-owner, even from well-understood economic considerations, is much more likely to expend something on the education of a slave who shows ability than on that of a beggar child.” According to Marx, this was the import of Hugo’s chapter on “freedom.” The same “frivolous shamelessness” appeared in the succeeding chapters on marriage (though Marx’s commentary was deleted by censors) and education, which are likewise treated on the positive and animal level. These views seemed to follow logically from the proposition that Marx extracted on civil law and cited with evident incredulity, that the “necessity of civil law in general is imaginary.”

In Marx’s view the full iniquity of Hugo’s concept of law appeared in his chapter on constitutional law. Here the convergence between the authoritarian methods of traditional jurisprudence and the authoritarian assumptions of conservative government was complete. “It is a holy duty of conscience to obey the authorities in whose hands power lies.” To this characterization of Hugo’s position Marx added the gloss, “Has not Hugo proved that man can cast off even the last fetters of freedom, namely, that of being a rational being?” How, then, could his successors claim to be “the legislators of our times?” Though only Karl Haller, Friedrich Stahl, and Heinrich Leo were mentioned, Marx was again obviously thinking of his mentor Savigny, then occupied with revising the Prussian laws. In general, the historical school worked out of that great storehouse of German erudition in which Marx himself had begun, and to this they added trappings from modern fashions of mysticism, romanticism, and speculation. “Truly, however,” Marx declared, “little criticism is needed to recognize behind all these fragrant modern phrases the dirty old idea of our enlightener of the ancien regime and to recognize behind all the extravagant unctuousity his dissolute frivolity.” This idea, which betrayed the ideals of ancient jurisprudence as well as modern social conscience, was that law was mere animal law, not human but “positive” law, and its implication was “the right of arbitrary power.”

The result of this line of criticism became clearer a year later when Marx, carrying out his resolution to place his arguments on a more philosophical basis, wrote an extended critique of Hegel’s philosophy of law. These pages contain no poetry (though begun during the first months of his marriage to Jenny von Westphalen), little idealism in any sense, and a large amount of convoluted and ill-humored philosophizing; but old juridical issues figured prominently, and echoes of old scholastic quarrels can be heard. Marx was still incensed at Hugo and his progeny, that “school which legitimates the baseness of today by the baseness of yesterday . . . , a school to which history shows only its posterior . . . ” (punning on the mindless empiricism, the *a posteriori* assumptions of the historical school).³¹ In the introduction to his

³¹ Marx, “Zur Kritik der Hegel’schen Rechtsphilosophie: Einleitung,” in *Marx-Engels Gesamtausgabe*, 1, pt. 1: 609 (*Collected Works*, 3: 177), published in *Deutsch-Französische Jahrbücher* in 1844.

critique, Marx broadened his attack by criticizing both law and religion (here stigmatized with the famous phrase “opium of the people”) as obstacles to the “common good.” In this way jurisprudence seemed explicitly to fall victim to Marx’s growing hatred of “all the gods” of established society. Guilty by association, too, was the political constitution (*politische Verfassung*), which Marx referred to as “the religion of national life, the heaven of its generality over against the earthly existence of its actuality.”

For Marx, then, Hegelian philosophy had come to be another manifestation of the “metaphysics of law” and of the fallacies both of idealism and of the historical school. On the one hand, Hegel distorted social reality with an abstract and authoritarian system, and, on the other, he continued to accept uncritically the positive and subhuman, “animal” law of the old regime. The bases of this law were the caste divisions (*Stände*) that turned man, according to Marx, “into an animal that is directly identical with its functions. The Middle Ages are the animal history of human society, its zoology.”³² Hegel assumed an abstract and transcendent state and a concept of “sovereignty” that led him into triviality and tautology; yet he accepted without question the status quo of German political organization. His remarks on the executive, for example, “could stand word for word in the Prussian common law.” What Hegel presented was not a system of philosophy, it might be concluded, but rather a system of “ideology” in a classically pejorative sense.

The fundamental contradiction in Hegel’s *Rechtsphilosophie*—that between a received juridical system and the common good that ought to inform society—had troubled Marx since his first year at the University of Berlin. In Hegel’s case it took the form of opposing the state to “the spheres of civil law and personal welfare, the family and civil society.” Once again the central question concerned private property and, particularly primogeniture, which isolated property from the family as well as from civil society.³³ Marx pointed out that Roman law—although it provided for slavery, right of conquest, and class conflict—had never confused private property with political power. By keeping the *jus privatum* separate from the *jus publicum*, it had also prevented the sovereign will (the *imperium*) from becoming hereditary. Germanic law was quite different. Hegel epitomized the view that alienated private property—that is, inalienable landed property—from its “social roots” and from public wealth and that identified it with the ruling establishment. “The political constitution at its highest point is the constitution of private property,” Marx declared. “The supreme political conviction is the conviction of private property.” Here was another god to hate: “Primogeniture is private property become a religion to itself, lost in itself, elated by its own independence and power.” It was “crass stupidity.”

³² Marx, “Kritik des Hegel’schen Staatsrechts,” in *Marx-Engels Gesamtausgabe*, 1, pt. 1: 499 (*Collected Works*, 3: 81); written in 1843, it was not published until the twentieth century. See the commentary by Joseph O’Malley in Marx, *Critique of Hegel’s ‘Philosophy of Right,’* ed. and trans. O’Malley and A. Jolin (Cambridge, 1970).

³³ Marx, “Kritik des Hegel’schen Staatsrechts,” in *Marx-Engels Gesamtausgabe*, 1, pt. 1: 518 (*Collected*

MARX HAD NOT, AT THIS POINT, fully developed his conception of class conflict, but it is evident that he had the makings of his theory. "Private property is the general category of the general political bond," he argued; but at the same time it was separated from public wealth, which is to say the common good, and this contradiction seemed to discredit the very foundations of jurisprudence and traditional political philosophy. Marx explained the rejection of his academic heritage in this way:

The criticism of the German philosophy of state and law, which attained its most consistent, richest, and final formation with Hegel, is both a critical analysis of the modern state and of the reality connected with it, and the resolute negation of the whole German political and legal consciousness as practised hitherto, the most universal expression of which raised to the level of a science is the speculative philosophy of law itself.

This marked a watershed in Marx's thought, for as he had come to realize, "The criticism of the speculative philosophy of law turns not toward itself, but toward problems which can be solved only by one means—practice."³⁴ The first problem that came to mind was the relationship between industry and the political world. Some such aspect of "modern politico-reality" Marx proposed to substitute for the philosophy of law as his proper field of investigation.

So it was that Marx came to believe that the grounds for discussion of true philosophy had to be not formal rules of law but realities of possession and property, that is, what the English and French called political economy and what the Germans, more primitively and unscientifically, called national economy. Society had to be comprehended in terms not of its legal superstructure, in other words, but of its material base. This shift in Marx's thought coincided with a physical transition from Germany, a regime which was "decaying and destroying itself," to France, the promised land of rationality and revolution. There "Charles" Marx found a new vehicle for his increasingly intemperate views, the *Deutsch-Französische Jahrbücher*, and from its editorial board he looked forward to "the commencement and continuance of the new era we are now entering."³⁵ Marx's final rejection of the metaphysics of law and his conversion to the antithetical science of economics were accomplished in this Parisian period and are reflected especially in his critique of James Mill and the *Economic and Philosophic Manuscripts* of 1844, which has been the basis for so much discussion of so-called socialist humanism in Marx, a revisionist view that does not seem to have added much to the strictly historical appreciation of Marx's thought.

In any case, even in his revolutionary phase Marx had not yet finished with the old jurisprudence. "The first criticism of any science," he later acknowl-

Works, 3: 98).

³⁴ Marx, "Zur Kritik der Hegel'schen Rechtsphilosophie," in *Marx-Engels Gesamtausgabe*, 1, pt. 1: 613 (*Collected Works*, 3: 181).

³⁵ Marx, Draft Program of the *Deutsch-Französische Jahrbücher*, published for the first time in *Collected Works*, 3: 181.

edged, "is necessarily influenced by the science it is fighting against"³⁶ This position can be seen not only in his assumptions about the social functions of property but also in his celebrated view of "alienation," a concept which again has a legal as well as a Hegelian background, relating specifically to the transfer, surrender, or loss of patrimony or (in a political context) domain. "Alienation of domain" (or of sovereignty) was one of the most bitterly resented abuses of the old regime, representing as it did the most fundamental betrayal of the commonwealth. And, as in public law, so in the private sphere the effects of what civilians called *alienatio* were destructive. As Marx expressed it, "the loss or surrender of private property [*Entäußerung oder Entfremdung des Privateigentums*] is alienation of man, as it is of private property itself."³⁷ In the Roman experience, which continued to be Marx's principal model, the consequence of such alienation was the loss of identity, of citizenship, and, indeed, of the entire basis for social "belonging." From the point of view of political economy, it signified the separation of labor from landed wealth (the first form of capital), the need for a measure of exchange value—that is, money. Alienated from the natural wealth of the tribal community, men were "bound to life" by this "universal agent of separation," as Marx called money.³⁸ But the fundamental form of "alienation" was not that Hegelian state of inner isolation that has monopolized the attention of students of Marx. Rather, paralleling Marx's own shift from idealistic to materialistic premises, it was estrangement from *property*—a problem that was widely discussed by civil lawyers, especially with regard to the *ager publicus*, that original common land of the Romans that so fascinated Marx and that continued to concern him in *Das Kapital*.

For Marx the juridical fictions underlying such alienation were continuously reinforced by the fallacies of idealism, and he carried on the attack against this two-faced evil over the next two years, especially in the *Holy Family* and the *German Ideology*. The "critical criticism" of the neo-Hegelian brothers Edgar and Bruno Bauer preserved the errors of Hugo by confusing fact and law, objective conditions and subjective consciousness. Edgar Bauer, for example, seemed to make mystical nonsense of Pierre-Joseph Proudhon's political-economic question, "What is property?"³⁹ On the other hand, Marx rejected the "illusions of the lawyers," who confused the Roman view of simple use and consumption (*jus utendi et abutendi*) with property, which was defined in terms of abstract and legalistic titles irrespective of any requirement of physical possession and which was, therefore, a major source of class

³⁶ Marx, *Die Heilige Familie*, in *Frühe Schriften*, ed. H. J. Lieber and P. Furth (Stuttgart, 1971), 1: 697 (*Collected Works*, 4: 31).

³⁷ Marx, Comment on James Mill, *Elements of Political Economy*, in *Marx-Engels Gesamtausgabe*, 1, pt. 3: 531 (*Collected Works*, 3: 218). The varieties of *alienatio* are taken up in all textbooks of Roman law; see in particular Adolph Berger, *Encyclopedic Dictionary of Roman Law* (Philadelphia, 1953). Also see Peter Riesen-berg, *Inalienability of Sovereignty in Medieval Political Thought* (New York, 1956). For some recent studies, see Bertell Ollman, *Alienation: Marx's Conception of Man in Capitalist Society* (Cambridge, 1971); and Joachim Israel, *Alienation: From Marx to Modern Sociology* (Boston, 1971). Neither of these, however, deals with the legal dimension.

³⁸ Marx, *Ökonomisch-Philosophische Manuskripte*, in *Frühe Schriften*, 1: 633 (*Collected Works*, 3: 324).

³⁹ Marx, *Die Heilige Familie*, in *Frühe Schriften*, 1: 697 (*Collected Works*, 4: 31).

division. The “juridical illusion” was to reduce law to—or, rather, to inflate it into—an expression of abstract will, just as idealist philosophers expressed social relationships as “ideas.” The Roman view of property was closer to reality: the relationship was not a legal fiction but the simple right of an individual to a piece of property, landed or movable, a right enjoyed by every Roman citizen (*dominium ex jure Quiritum*), stemming from *possessio*.⁴⁰ To neglect this was to fall into a confusion akin to that of Hegel’s phenomenology, which Marx summarized cryptically in his notebook in November 1844: “Self-consciousness instead of man. Subject—object.” And alienation was similarly subjectivized: “Abolition of estrangement is identified with abolition of objectivity.”⁴¹ For Marx, on the contrary, what needed abolishing was not the conscious state of isolation but the material condition of propertylessness, which left men open to exploitation.

In this way Marx continued to grapple with his intellectual heritage and to work on the problem that he stated in a note to the *German Ideology* as “Why ideologists”—including jurists, religionists, politicians, and moralists—“turn everything upside down.” All of them stood in the way of the positive, materialistic, historical, and, ultimately, revolutionary view of society that Marx was seeking. Nor did political economists escape his ire. As he had rejected the “metaphysics of law,” so he also turned against the “metaphysics of political economy,” a phrase that he applied in particular to the views of Proudhon.⁴² What first distinguished man from animals, according to Marx, was not consciousness or speech or religion as ideologists imagined, but rather production. The subsequent emergence of property and legal distinctions attached thereto marked the true beginning of history. “Civil law developed simultaneously with private property from the disintegration of the natural community.” This process also continued in the modern European context. “With the exception of England it proceeded everywhere on the basis of the Roman Codex,” he continued. “Even in England Roman legal principles had to be adopted to further the development of civil law, particularly in regard to movable property.”⁴³ The most recent stage of this process was the transformation of movable property into capital, which has fallen into the hands of the legally dominant class with title to, though not necessarily possession of, this wealth.

At the end of this train of thought lay a number of the most fundamental elements of mature Marxism, not only concepts of class struggle and communism but also the aim of “changing the world” rather than merely interpreting it, as did the jurists, philosophers, and “bourgeois” economists.⁴⁴ But even as communism was beginning to haunt the conscience of bourgeois Europe, Marx himself continued to be haunted by the old learning, especially that embodied in civil law. Perhaps most essential was the notion of an

⁴⁰ Marx, *Die Deutsche Ideologie*, in *Frühe Schriften*, 2: 92–97 (*Collected Works*, 5: 89–93).

⁴¹ Marx, Manuscript Note, in *Collected Works*, 4: 665.

⁴² Marx, *Das Elend der Philosophie*, chap. 2, in *Frühe Schriften*, 2: 738 (*Collected Works*, 6: 161).

⁴³ Marx, *Die Deutsche Ideologie*, in *Frühe Schriften*, 2: 93 (*Collected Works*, 5: 91).

⁴⁴ Marx, *Thesen über Feuerbach*, in *Frühe Schriften*, 2: 4 (*Collected Works*, 3: 5).

original communal property (the *ager publicus*) that ancient Romans cultivated, he argued, not as dispossessed laborers, but as owners (*nicht als Arbeiter, sondern als Eigentümer*) for purposes of subsistence (*Erhaltung des Einzelnen*), not the creation of exchange-value (*Wertschöpfung*).⁴⁵ In this revealing passage of the *Grundrisse*, Marx lapsed into English to make his point: “Das Individuum ist placed in such conditions of gaining his life as to make not the acquiring of wealth his object, but self-sustenance [*sic*] and its own reproduction as a member of the community” To underscore the practical character of Roman arrangements, Marx quoted in this connection the great historian Barthold Georg Niebuhr, who remarked of Numa Pompilius that, after being assured of divine approval of his election, he turned his attention not to religious but to human business (*nicht Tempeldienst, sondern menschlich*). “He distributed the land conquered in war by Romulus and left to be occupied All great law-givers, and above all moyses, founded the success of the arrangements for virtue, justice, and good morals upon landed property, or at least on secure hereditary possession of land, for the greatest possible number of citizens.” Afterwards, this tribal and communal condition was destroyed by migration, conquest, and the differentiation into higher and lower tribes (*gentes*), and so began the period of recorded history—“the history of class warfare,” according to the opening words of the *Communist Manifesto*.

Long before the experience of revolution, then, Marx realized that his pursuit of true philosophy had to take a different path from those of his juristic and philosophic mentors. By 1845 he was already contemplating a “critique of politics and economics” to rival the systems of the legal tradition, though he did not begin to lay out “the whole shit,” as he called it, until the late 1850s.⁴⁶ Like the treatise on jurisprudence planned in adolescence, this work began with the sphere of private law. This time, however, the target was not property but capital, and the basic issue was not the theoretical “condition of persons,” as in civilian convention but rather with exchange-value and money, which had made such abstract questions irrelevant in Marx’s mind, and then with landed property and wage labor. Thereafter, again as in the youthful work, Marx intended to move into the public sphere, beginning with the state seen, of course, from a broader perspective (*naturae politico-economicae*, as he characterized his approach to Engels), followed by discussions of international trade and the world market. This enterprise, of course, constitutes another story entirely and must be understood in a very different context.

IN GENERAL, THE PURPOSE OF THESE REMARKS has not been to resurrect another “Young Marx,” nor even to revise any particular interpretation of the old one;

⁴⁵ Marx, *Grundrisse der Kritik der Politischen Ökonomie* (Berlin, 1953), 378–80, “Formen, die der kapitalistischen Produktion vorhergehen,” translated by J. Cohen as *Pre-Capitalist Economic Formations* (New York, 1965), 72–74.

⁴⁶ Marx to Engels, Apr. 2, 1858, in *Karl Marx/Friedrich Engels Briefwechsel* (Berlin, 1849), 2: 383–87. Also see Marx to Engels, Jan. 7, 1851, in *ibid.*, 1: 152.

it has merely been to open up a neglected perspective on the genesis and early development of Marxist social thought. Marxism has, of course, generated a massive scholasticism—glosses, commentaries, and interpretations by hostile as well as friendly students—but little of this has even attempted to offer a strictly historical construction. Even when various ideological totems and taboos have been avoided, the tendency has been to place Marx exclusively in the traditions of classical economics and philosophy, especially of German idealism. Although this approach is not improper in itself, it is insufficient as a basis of appreciating the richness of Marx's intellectual heritage. His conception of the world emerged from a surpassingly deep and wide-ranging assault upon Western humanistic, historical, and legal scholarship, going far beyond the names that have become familiar from general post-Marxian surveys of philosophy and economic, social, and political thought.⁴⁷ Only by entering into the less familiar terrain of old regime scholarship can one hope to appreciate fully the extent of Marx's intellectual base and of the conceptual struggles underlying his mature philosophy.

Within Marx's intellectual heritage, jurisprudence may not occupy a central position, but it does have a kind of temporal priority. The young Marx was steeped in legal erudition and his thoughts about social problems shaped by legal issues, especially those attendant upon the historical school and its opponents, though students of Marx have not tried to follow up his early enthusiasms and distastes.⁴⁸ What is more, jurisprudence provided not only an early focus for Marx's studies but a system of thought that continued to possess at least a negative importance. If it did not furnish him with the "true philosophy" he had been taught to expect, it did give him a satisfying target—a comprehensive secular religion and a paradigm of idealist social thought, in a pejorative sense of "ideology"—for his atheistic as well as his materialistic and socialistic urges. If it did not contain the divine fire, the means of understanding and of changing human society, it did offer him a socially meaningful way of venting his Promethean hatred of "all the gods" and of finding a way out of the trap of idealism. The juridical problems of possession and property led directly to the key, which was the iconoclastic new discipline of economics. In this Marx was to find his Promethean fire, the *arche* of his own true philosophy, perhaps of his religion, and a way of exorcizing all the gods that had oppressed mankind—an enterprise altogether worthy of classical jurisprudence as conceived of by the very young Marx.

⁴⁷ Survivals of old concerns are also illustrated in Marx's later studies of anthropology, a subject originally part of academic jurisprudence as well as philosophy; see notes 9 and 30, above, and *The Ethnological Notebooks of Karl Marx*, ed. Lawrence Krader (Assen, 1974).

⁴⁸ The importance of Marx's legal studies and his associations with Gans and Savigny have been pointed out by his biographers, especially Isaiah Berlin and August Cornu; but it has been so uniformly neglected by interpreters of the young Marx and of the documentable genesis of his thought up to 1842 who have favored the specifically Hegelian connection that I have not thought it relevant to refer to the works of Georg Lukács, Herbert Marcuse, David McClellan, and others, much less to analyses of the sources of Marxian economic and formally philosophic thought. For a more general discussion, see J. E. Seigel, "Marx's Early Development: Vocation, Rebellion, and Realism," *Journal of Interdisciplinary History*, 3 (1972-73): 475-508, which will appear in revised form in his forthcoming book.

The Churchill-Stalin Secret “Percentages” Agreement on the Balkans, Moscow, October 1944

ALBERT RESIS

AMONG ALL THE BOOKS ON WORLD WAR II published since 1945, none can challenge the supremacy of Winston Churchill’s six-volume *The Second World War*. A unique blend of memoir, document, and narrative, his work—published between 1948 and 1953—continues to dominate the field. And rightly so, unmatched as the work is in color, sweep, and power. In all the history of diplomacy surely no passage is more dramatic or more shocking than Churchill’s account of the meeting he held with Joseph Stalin in the Kremlin on the evening of October 9, 1944. Churchill tells us that, immediately after a brief discussion on Poland, he and Stalin moved to Balkan problems. No paraphrase can do justice to Churchill’s own account. Churchill said to Stalin,

“Let us settle about our affairs in the Balkans. Your armies are in Rumania and Bulgaria. We have interests, missions, and agents there. Don’t let us get at cross-purposes in small ways. So far as Britain and Russia are concerned, how would it do for you to have ninety per cent predominance in Rumania, for us to have ninety per cent of the say in Greece, and go fifty-fifty about Yugoslavia?” While this was being translated I wrote out on a half-sheet of paper:

Rumania	
Russia	90%
The others	10%
Greece	
Great Britain (in accord with U.S.A.)	90%
Russia	10%
Yugoslavia	50-50%
Hungary	50-50%
Bulgaria	
Russia	75%
The others	25%

I pushed this across to Stalin, who by then had heard the translation. There was a slight pause. Then he took his blue pencil and made a large tick upon it, and passed it back to us. It was all settled in no more time than it takes to set down.

The author wishes to express his appreciation to the Photographic Department of the Public Record Office, London, for the kind assistance in photocopying PRO materials used in this article.

Then, Churchill reports, there was a long silence while the penciled paper lay on the center of the table. "At length I said, 'Might it not be thought rather cynical if it seemed we disposed of these issues, so fateful to millions of people, in such an off-hand manner? Let us burn the paper.' 'No, you keep it,' said Stalin."¹

Churchill leaves the distinct impression that Stalin fully agreed with this arrangement. Not until 1958, five years after Churchill published his account, did Soviet authorities or historians comment on this claim. Then they bitterly denied—and continue to deny—that Stalin had accepted this imperialist proposal. According to I. Zemskov, a leading Soviet diplomatic historian, the Soviet record of the Churchill-Stalin meeting of October 9, 1944, states, "Churchill announced that he had prepared a rather dirty, crude [*grubyi*] document that showed the distribution of Soviet and British influence in Rumania, Greece, Yugoslavia, and Bulgaria. The table was drawn up by him to show what the British think about the question." Dividing southeastern Europe into spheres of influence was, according to this Soviet historian, an obsession of Churchill and the British government. Churchill actually made the proposal. But Churchill's assertion that Stalin had given his assent was belied by the absence of a copy of Churchill's table in the Soviet record. Nor did Churchill and Stalin in their subsequent meetings ever again allude to the alleged deal.² In short, the Soviets claim that Stalin treated the proposal as unworthy of his notice.

Churchill's account and the Soviets' repudiation of it prompt serious historical questions. Was the agreement in fact concluded as Churchill described it? If the agreement was so concluded, what exactly did its sibylline terms mean—percentages of what? What was the intended duration of the agreement? What, if any, was the character and extent of Franklin D. Roosevelt's involvement? Answers to these questions would throw light not only on the reliability of Churchill's account but also on wartime diplomacy and the immediate causes of the Cold War in southeastern Europe. In addressing

¹ Churchill, *The Second World War*, vol. 6: *Triumph and Tragedy* (New York, 1962), 196–97.

² Zemskov, "O tak nazyvaemom 'razdele' Iugoslavii na 'sfery vliianii'," *Mezhdunarodnaia zhizn'*, no. 8 (1958): 74. Zemskov's article, based on part of the Soviet record of the Churchill-Stalin meeting, offers no convincing proof that Stalin rejected Churchill's offer. The article was a Soviet reply to Yugoslav leaders who charged in 1958 that the Soviet Union had entered into an imperialist agreement with Britain to divide the Balkan nations into spheres of influence; *ibid.*, 72–73. The Soviet record of the Churchill-Stalin conversations has not been published. Until the Zemskov article, Soviet diplomatic and military historians got around the embarrassment of the "percentages" agreement by simply ignoring the Churchill-Stalin meetings. See, for example, G. A. Deborin, *Vtoraia mirovaia voina: voenna-politicheskii ocherk* (Moscow, 1958), translated by Vic Shneerson as *The Second World War: A Politico-Military Survey* (Moscow, n.d.); L. N. Ivanov, *Ocherki mezhdunarodnykh otnoshenii v period vtoroi mirovoi voiny* (Moscow, 1958); I. F. Ivashin, *Ocherki istorii vneshnei politiki SSSR* (Moscow, 1958); and S. Boratynskii, *Diplomatiia perioda vtoroi mirovoi voiny* (Moscow, 1959), a work by a Polish historian translated into Russian. Soviet scholars have followed Zemskov in contending that Stalin spurned Churchill's offer. See, for example, V. I. Israelian, *Diplomaticheskaiia istoriia velikoi otechestvennoi voiny* (Moscow, 1959), 260, and *Antigitterovskaia koalitsiia* (*Diplomaticheskoe sotrudnichestvo SSSR, SShA, i Anglii v gody vtoroi mirovoi voiny* (Moscow, 1964), 472; V. Trukhanovsky, *British Foreign Policy during World War II* (Moscow, 1970), 407–08; *Istoriia diplomatii*, 4 (Moscow, 1975): 499; and *Istoriia vneshnei politiki SSSR*, 1: 1917–1945 (Moscow, 1976), 467. Recent Soviet biographies of Churchill and Eden simply ignore the meeting. See, for example, V. Trukhanovsky, *Uinsten Cherkhill: Politicheskaiia biografiia* (Moscow, 1969), and *Antoni Eden: Stranitsy angliiskoi diplomatii. 30–50-e gody* (Moscow, 1974), 256–58.

these questions, the inaccessibility of the Soviet archives and the non-publication of the relevant Soviet documents preclude definitive answers. Nevertheless, the large body of memoir literature now available and, above all, the release to the public of the top-secret British record of the Churchill-Stalin meetings afford a much clearer picture of the Moscow talks of October 1944 than that previously possible.³

WHAT IS CHURCHILL'S OWN EXPLANATION of the meaning of the agreement? Troubled by the shocking impression the account might make on the reader, Churchill took great pains to persuade us that it was not a long-term, spheres-of-influence arrangement. Churchill claimed that he and Stalin "were only dealing with immediate war-time arrangements" and that all major questions were reserved for the postwar peace table.⁴ On October 11, Churchill wrote to Roosevelt to reaffirm that agreements reached at Moscow would in no way commit the president. But it is absolutely necessary, he wrote, that

[Stalin and I] should try to get a common mind about the Balkans, so that we may prevent civil war breaking out in several countries, when probably you and I would be in sympathy with one side and U.J. [Stalin] with the other. I shall keep you informed of all this, and nothing will be settled except preliminary agreements between Britain and Russia, subject to further discussion and melting down with you. On this basis I am sure you will not mind our trying to have a full meeting of minds with the Russians.⁵

On the same day Churchill drafted a letter and a memorandum to Stalin, which set forth Churchill's interpretation of the "percentages" agreement.

³ The record—printed presumably for top-level, restricted circulation—is titled "Anglo-Russian Political Conversations at Moscow, October 9–October 17, 1944." It is deposited at the Public Record Office as Prem. 3 434/4 9565. An Associated Press dispatch from London reported that release of the papers in 1973 was delayed eighteen months owing to the disorder in which the papers were found. Moreover, a section dealing with political conversations between Churchill and Stalin was missing, and officials could not explain the disappearance; *New York Times*, August 5, 1973. This record of the talks does not include Churchill's "percentages" table, which is, however, in the Churchill Papers; E. L. Woodward, *British Foreign Policy in the Second World War*, 3 (London, 1971): 150, n. b. In 1961 F. W. D. Deakin, countering Soviet assertions that there was no agreement between Stalin and Churchill on spheres of influence, stated that he had seen the relevant document in the archives; it showed the existence of an "understanding," if not a contractual treaty, between Stalin and Churchill on this question. On the British side, Deakin said, it was an attempt to discern "in good faith what the policy of the Russians would be in certain countries . . ." Deakin in *European Resistance Movements, 1939–1945*. Proceedings of the Second International Conference on the History of the Resistance Movements held at Milan, March 26–29, 1961 (New York, 1964), 644–45. But in July 1973 Deakin said that Churchill's "percentages" table was only a trial balloon intended to elicit from Stalin information on where the Russian army was going and was not intended as an agreement to divide up the Balkans; Deakin in Phyllis Auty and Richard Clogg, eds., *British Policy towards Wartime Resistance in Yugoslavia and Greece* (London, 1975), 247–48. The British interpreter for Churchill and Eden at the Moscow meetings is silent about the content of the conversations; A. A. Birse, *Memoirs of an Interpreter* (New York, 1967). Anthony Eden adds nothing significant to Churchill's account; *The Memoirs of Anthony Eden, Earl of Avon*; vol. 2: *The Reckoning* (Boston, 1965). C. L. Sulzberger claims that he has a photocopy of the Churchill "percentages" table; see *A Long Row of Candles: Memoirs and Diaries, 1934–1954* (New York, 1969), photograph following 525.

⁴ Churchill, *Triumph and Tragedy*, 197. Also present were Foreign Secretary Anthony Eden and British Ambassador to the Soviet Union Sir Archibald Kerr. People's Commissar for Foreign Affairs Viacheslav Molotov accompanied Stalin. Ambassador Averell Harriman, President Roosevelt's observer, was not invited to attend this meeting.

⁵ *Ibid.*, 198.

Churchill did not, however, send the letter, "deeming it wiser to leave well alone," but he did reprint it to present an "authentic account" of his thoughts.⁶ He pointed out to Stalin that any agreement between them should be acceptable to the United States and could only be preliminary to agreement at the peace table. Nevertheless, Britain and the Soviet Union were bound by a twenty-year alliance, and it was imperative that they reach "understandings" and in some cases "agreements" that would smooth the way through present emergencies to a lasting peace. The "percentages" were no more than "a method by which in our thoughts we can see how near we are together" and then decide upon the steps required to attain full agreement. As Churchill understood the agreement, neither side should impose its system on another people. The British, however, had special obligations toward the kings of Greece and Yugoslavia. Of course, the peoples, once liberated, should be free to choose any kind of government they wanted—except fascism. But, once tranquility had been restored, Britain and Russia should not have to worry about or interfere with these governments. Finally, he wrote, the "percentages" were designed to indicate the degree of interest each side took in these countries "with the full assent of the other" and subject to the approval of the United States.⁷

Writing to the War Cabinet on October 12, 1944, Churchill denied that the "percentages" were intended to prescribe the number of members sitting on commissions for the different Balkan countries or to set up a rigid system of spheres of interest. But it was only right that Britain show "particular respect" to the Russians' desire to take the lead in Rumania, which had attacked the Soviet Union, and in Bulgaria, with which the Russians had ancient ties. Similarly, the Soviet Union was prepared to concede to the British the same role in Greece that Britain conceded to the Russians in Rumania. Thus, a civil war in Greece with Britain and Russia each taking opposing sides could be prevented. The fifty-fifty agreement for Yugoslavia entailed joint action on agreed policy there to foster a united Yugoslavia. Since the Soviet armies were in Hungary, the Soviet Union would naturally take the "lead" there, subject to the assent of Great Britain and probably the United States. Nevertheless, the Balkan arrangement was "only an interim guide for the immediate wartime future. . . ."⁸

Thus, after implying that he and Stalin had entered into an agreement worthy of eighteenth-century monarchs or nineteenth-century empire-builders, Churchill strained to persuade his associates and his readers that the agreement was not what it seemed. Instead, this temporary, wartime measure merely delimited zones of military and political responsibility and entailed neither partition of third countries nor old-fashioned spheres of influence. And any agreement he and Stalin reached was subject to approval by the

⁶ *Ibid.*, 200. He did not reprint the memorandum, nor did he send the letter, because Ambassador Harriman told him that President Roosevelt and Secretary of State Cordell Hull would repudiate it; W. Averell Harriman and Elie Abel, *Special Envoy to Churchill and Stalin, 1941-1946* (New York, 1975), 358.

⁷ Churchill, *Triumph and Tragedy*, 200-01.

⁸ *Ibid.*, 202-03.

president of the United States. The British record shows, however, that both Churchill's account of the "percentages" agreement in *Triumph and Tragedy* and the Soviet claim that Stalin had tacitly dismissed the proposal do not correspond with the evidence now available. Churchill and Stalin did reach an agreement; but it was an agreement both on less and on more than Churchill revealed in his published account: the deal was not concluded with the extreme ease, with the entire purport, or with the specific reservations claimed by Churchill.

THE BRITISH RECORD REPORTS that Churchill, in his meeting with Stalin in the Kremlin on October 9, 1944, after a brief exchange with Stalin on the Polish question,⁹ turned to the Balkans. There were two countries, he said, in which the British had a particular interest. Rumania "was very much a Russian affair" and the armistice terms that the Soviet government had proposed to Rumania in September were "reasonable and showed much statecraft in the interests of general peace in the future." But Greece was another matter. "Britain must be the leading Mediterranean power," Churchill said, "and he hoped Marshal Stalin would let him have the first say about Greece" in the same way as Marshal Stalin had in Rumania. The British government would of course keep the Soviet government informed on events in Greece.¹⁰ Stalin for his part sympathized with Churchill for the hardships Britain suffered owing to the severance of Britain's Mediterranean communications by the Germans. Stalin agreed that, in order to safeguard these lines, Britain "should have the first say in Greece." Having traded off Rumania for Greece, Churchill said it would be better to avoid the phrase "dividing into spheres" because that might shock the Americans. "But as long as he and Stalin understood each other he could explain matters to the President."¹¹ This trade, in effect, extended into perpetuity the Anglo-Soviet agreement of May 1944, which allowed Britain the predominant voice in Greek affairs and the Soviet Union the predominant voice in Rumanian affairs—an agreement to which in June President Roosevelt had consented to give a "three months trial period," after which it would be reviewed by the three great powers.¹²

Warming to his subject, Churchill proceeded to sound out Stalin on still wider spheres of interest. Regarding disputes involving the great powers in the future United Nations Organization, Churchill said that he now favored the Soviet view: a great power should not be excluded from a vote in a dispute to

⁹ "Anglo-Russian Political Conversations," 4-5. The record reveals little that is new on the Polish question, except to show that Churchill was far more exasperated with the anti-Soviet predilections of the London Poles than was generally believed. He even expressed satisfaction that General Tadeusz Bor, the commander of the Warsaw uprising who had just surrendered to the Germans, would no longer be a problem for Churchill and Stalin. "The Germans were looking after him." *Ibid.*, 4-5.

¹⁰ *Ibid.*, 5.

¹¹ *Ibid.*

¹² On this agreement, see *Foreign Relations of the United States: Diplomatic Papers* (hereafter cited as *FRUS*), 1944, 1: 610-11, 5: 112-21; Churchill, *Triumph and Tragedy*, 62-69; and Cordell Hull, *The Memoirs of Cordell Hull*, 2 (New York, 1948): 1451-58.

which it was a party. As a reason for rejecting the opposing American view, Churchill cited the possibility that China might demand that Britain relinquish Hong Kong.¹³ Stalin did not say a word. But Churchill's message was clear: if the Soviets would not oppose Churchill's efforts to recover the empire that Britain temporarily lost to Japan, Churchill would not oppose Stalin's efforts to realize the legitimate territorial aspirations of the Soviet Union. In these matters, both had a common interest against the United States, which, in the person of Roosevelt, hoped to reduce the British Empire for the benefit of a Nationalist China and which continued to withhold recognition of the Soviet territorial gains of 1939-40. Readiness to support each other on a global scale meant that Churchill and Stalin could surely work out problems of southeastern Europe, where their political allies were already clashing and their own armed forces might soon meet.

At the point at which Churchill and Stalin began their bargaining over spheres of influence in southeastern Europe, there is a gap in the British record. It simply shows that after lengthy discussion "it was agreed that as regards Hungary and Yugoslavia each of the two Governments were equally interested; that Russia had a major interest in Rumania; and that Great Britain was in the same position with regard to Greece."¹⁴ Despite Churchill's account, which reports agreement to a seventy-five-twenty-five division for Bulgaria in favor of the Russians, he and Stalin in fact failed to reach any agreement on that country. Churchill conceded Soviet primacy in Bulgaria, but he insisted that Britain be given a greater voice in Bulgarian affairs than in Rumanian. The dispute widened to include Turkey and the Dardanelles, when Stalin, for the first time in the conversations, took the initiative on a territorial question. He insisted on revision of the Montreux Convention on the Straits, because, "if Britain were interested in the Mediterranean, then Russia was equally interested in the Black Sea." Churchill assured Stalin that Britain no longer grudged Soviet Russia access to warm-water ports and to the great oceans and seas of the world. On the contrary, it was part of British friendship to help the Soviet Union. What changes did Stalin think were required in the Montreux Convention?¹⁵

Stalin declared that the entire convention should be scrapped, because it was pointed against Russia. The Soviet Union could no more tolerate a Turkish stranglehold on the Straits than Great Britain could tolerate an Egyptian stranglehold on the Suez Canal or the United States could tolerate a

¹³ "Anglo-Russian Political Conversations," 6; and *FRUS*, 1944, 4: 1017.

¹⁴ "Anglo-Russian Political Conversations," 6.

¹⁵ *Ibid.*, 6-7; and Churchill to Roosevelt, London, October 22, 1944, *FRUS*, 1944, 4: 1024. The British Foreign Office had argued in April 1944 that British interest lay in preventing the extension of Soviet influence toward the Straits and the Mediterranean. To this end a British military force should be sent to Greece to support the interim Greek administration; there ought to be British membership on the Allied Control Commission for Bulgaria; and a British force ought to be sent there to "show the flag" and, if necessary, to ensure Bulgarian evacuation of Greek and Yugoslav territory. But there could be no question of any British occupation of Bulgaria except in agreement with the Russians, and the same applied to Hungary. Failure to act would encourage the growth of zones of influence dividing Europe into rival camps led respectively by Great Britain and the Soviet Union. "Memorandum Prepared in British Foreign Office, April 17, 1944." *FRUS*, 1944, 1: 596-99.

Latin American stranglehold on the Panama Canal. Churchill conceded that the Montreux Convention was “inadmissible” and “obsolete”; Russia “had a right and moral claim” to free passage of the Turkish Straits. But Turkey had to be brought along gradually, to which Stalin agreed.¹⁶ Churchill and Stalin had moved closer to an understanding of the need to respect each other’s core security interests; but they still could find no fair division of influence for Bulgaria. Yet there is not the slightest evidence in the British record that Stalin concurred in, or agreed to, any comprehensive “percentages” arrangement; nor is there evidence that he indignantly spurned Churchill’s offers. In fact, a comprehensive informal understanding on the Balkans—except for Bulgaria—had been accepted by Stalin. Despite Churchill’s repeated assurances in *Triumph and Tragedy* that the arrangements concluded with Stalin in Moscow were only temporary, wartime measures and subject to approval by the president of the United States, the British record shows that neither Churchill nor Stalin attached any such reservations to the agreed terms. Thus, the two leaders’ informal understanding was exclusively bilateral and of unspecified duration.

What was the meaning of the understanding and what rights and obligations did it entail? First of all, Churchill said, he and Stalin “should do something to prevent the risk of civil war between the political ideologies” and to prevent the outbreak of little wars in the Balkan countries after the defeat of Hitler. Stalin agreed.¹⁷ Both Britain and the Soviet Union had the capability of creating a good deal of mischief in the sphere allotted to the other, where each had indigenous allies, friends, sympathizers, and secret agents. But given the power of its armies of occupation, the Soviet Union had less to fear in its sphere than did Britain in the Mediterranean. Moreover, pro-Soviet resistance forces in the British sphere were more powerful militarily than were pro-Western resistance forces in the Soviet sphere. Churchill was, therefore, much the weaker party in the bargaining. Even before the meeting with Stalin, Churchill had sought and secured from the Russians the assurance that they would not send their armed forces to Greece.¹⁸ Consequently, Britain was free to suppress the Greek resistance movement, which threatened the existence of the British-backed royal government. Partially reassured on Greece, Churchill was still haunted by the fear that the formidable antifascist resistance forces might seize power in Italy’s industrial north, still occupied by the Germans. Churchill, therefore, flatly asked Stalin “to soft-pedal the Communists in Italy and not to stir them up.” Let “pure” democracy decide whether the Italians wanted a republic or a monarchy.¹⁹

Again the talks had leaped beyond the Balkans. Stalin’s reply to Churchill is astounding evidence of how far Stalin was willing to go, verbally at least, in

¹⁶ “Anglo-Russian Political Conversations,” 7, 41–42.

¹⁷ *Ibid.*, 7.

¹⁸ Elisabeth Barker, *British Policy in South-East Europe in the Second World War* (London, 1976), 144; and see pages 380–81, below.

¹⁹ “Anglo-Russian Political Conversations,” 7.

sacrificing the revolutionary interests of Communist parties abroad for the sake of preserving Big Three unity. He said that

it was difficult to influence Italian Communists. The position of Communists differed in different countries. It depended upon their national situation. If Ercoli [Palmiero Togliatti, secretary-general of the Communist party of Italy] were in Moscow Marshal Stalin might influence him. But he was in Italy, where the circumstances were different. He could send Marshal Stalin to the devil. Ercoli could say he was an Italian and tell Marshal Stalin to mind his own business. . . . However, Ercoli was a wise man, not an extremist, and would not start an adventure in Italy.

Ercoli, Stalin assured Churchill, had said that he would collaborate with the king "if the King stood by the people."²⁰

So far Churchill and Stalin had agreed to recognize the other's primacy in the Mediterranean and the Black Sea areas respectively. For that very reason, Bulgaria, which had been both a Black Sea and a Mediterranean state, and Turkey, which still was both, posed great difficulties. Stalin turned to Bulgaria. He had just implied that he had no intention of communizing Italy, which lay in the British sphere. Now he also claimed that he had even put a stop to the communization of Bulgaria, which lay in the Soviet sphere. Bulgarian Communists, Stalin said, had proceeded to organize soviets in the wake of the Red Army, but the Red Army stopped them. The Bulgarian Communists had arrested the Bulgarian police, but the Red Army freed them.²¹ Bulgaria, after all, was a Black Sea country. Why did Britain demand a greater say in Bulgaria than in Rumania? The Soviet Union intended neither to attack Turkey by way of Bulgaria nor to treat Bulgaria too leniently, for that country had to be punished for her two wars on the side of Germany. Anthony Eden interjected that Britain had been at war with Bulgaria for three years and, accordingly, wanted a "small share" in the control of that country after Germany's defeat. Churchill suggested that Eden and Viacheslav Molotov thresh out the details of a settlement on Bulgaria, and Stalin agreed.²²

On the evening of October 10, Eden and Molotov met to work out the details of the "percentages" agreement reached by their chiefs the night before. The Russians now retracted their assent to a fifty-fifty agreement respecting Hungary. Molotov said that Stalin now proposed a seventy-five-twenty-five division, because Hungary "had been and always would be" a

²⁰ *Ibid.*, 7-8. Although Italy had capitulated in September 1943, Togliatti did not return to Italy from Moscow until April 1944. And Maxim Litvinov, Deputy Commissar for Foreign Affairs, told Ambassador Kirk that the Russians "do not want revolutions in the West, but if they happen we must approve." Kirk to Hull, Rome, September 9, 1944, *FRUS*, 1944, 3: 1149.

²¹ "Anglo-Russian Political Conversations," 8. Georgi Dimitrov, head of the Comintern (1935-43) and Bulgaria's most renowned Communist leader, did not return from Moscow to Sofia until November 6, 1945, more than a year after the liberation of Bulgaria. Meanwhile, he had been urging the Bulgarians to recognize that future peace depended primarily on preservation of "Big Three comradeship and collaboration." Therefore, it was vital for Bulgaria's future that Bulgarians foil "intrigues or actions aimed at rousing mutual suspicions and misunderstandings between the Allies," no matter their source. G. Dimitrov, "All For the Front," Moscow, September 28, 1944, in *Selected Works*, 2 (Sofia, 1972): 238. No doubt he had in mind the formation of Bulgarian soviets as an example of such disruptive action.

²² "Anglo-Russian Political Conversations," 8.

country bordering on the Soviet Union and because the Red Army had suffered such great losses in that country. The Russians had no territorial claim in Hungary, but they wanted to make certain that Hungary would not be on the side of a future aggressor.²³ Eden complained that the British were being presented with a series of *faits accomplis* in the Balkans. The British had saved Josip Broz Tito, yet the ingrate decamped to Moscow without notice. He was making an agreement with the Bulgarians to fight the Germans in Yugoslavia, despite British opposition to cobelligerency status for Bulgaria. British officers in Grecian Thrace were being mistreated by Bulgarian troops. Eden wanted the Russians to order the Bulgarians to treat the British representatives with respect. At first Molotov demurred. After all, the Soviet Union had agreed not to interfere in affairs beyond the borders of Bulgaria, especially in Greece. He finally consented, however, to speak to Marshal Stalin about the matter.²⁴ Next they turned to the question of the Bulgarian armistice. Eden agreed that terms ought to be worked out in Moscow, then coordinated in London with the Americans in the European Advisory Commission (EAC), the tripartite Allied body charged with such matters. American concurrence, he said, would be easy to obtain once Britain and the Soviet Union reached agreement. But, he warned Molotov, Britain could make no concession on one issue: the Bulgarian armistice. The British must have some share in the Allied Control Commission (ACC) for Bulgaria after the capitulation of Germany.²⁵

This marked a sharp break with previous British practice. Hitherto, both the United States and Great Britain had acquiesced in the Rumanian armistice (September 12, 1944) and the Finnish armistice (September 19, 1944), which gave the Soviet Union the decisive voice in each country both before and after the cessation of hostilities with Germany. The identical clause that the Russians had drafted for the Allied Control Commissions for Rumania and Finland provided that, until the conclusion of peace, the regulation of—and control over—the terms of the armistice would be vested in an Allied Control Commission operating “under the general direction and orders of the Allied (Soviet) High Command, acting on behalf of the Allied Powers.”²⁶ In short, the clause not only gave the Soviet Union the major voice in overseeing execution of the armistices; it also gave the Soviet Union the right to exercise this power in the name of its Allies. The Russians were determined to employ the same clause in the Bulgarian and Hungarian armistices.

At this point the United States made its first, decisive intervention in Balkan affairs during the war. For the Americans as well as the British now refused to allow the Russians to exercise unilateral control, in the name of the Allied Powers, over the future armistices with Hitler's last allies. The American draft article, which Eden pressed on Molotov, proposed that the Allied Control

²³ Eden and Molotov Meeting, October 10, 1944, *ibid.*, 10.

²⁴ *Ibid.*, 11. On October 10 Moscow instructed Marshal Tolbukhin to order Bulgarian forces in northern Greece to treat the British properly; *ibid.*, 16.

²⁵ *Ibid.*, 11.

²⁶ Andrew Rothstein, ed., *Soviet Foreign Policy during the Patriotic War*, 2 (London, 1946): 123–25 (text of the Rumanian armistice), 128–32 (text of the Finnish armistice).

Commission supervise the execution of the Bulgarian armistice "under the general direction of the Allied (Soviet) High Command." This clearly acknowledged Soviet supremacy in Bulgaria for the wartime period. After the cessation of hostilities in Europe and pending the peace treaty with Bulgaria, however, the commission would supervise execution of the armistice "according to the instructions of the Governments of the United States, Soviet Union and United Kingdom."²⁷ Such an article would ensure that the United States and Great Britain each would have a voice equal to that of the Soviet Union in controlling the Bulgarian armistice in the postwar period. In effect the Americans and the British began to insist that they have a greater voice in Bulgaria and Hungary than they had been allowed by the Russians in Rumania and Finland and a greater voice than the Russians had been allowed in control of the Italian armistice concluded the year before.

Molotov immediately challenged the feasibility and equity of the American draft of this article, number eighteen. In the Italian and Rumanian armistices, he said, the responsibility for Allied control was clear-cut. But the new approach recommended for Bulgaria during the postwar period would divide responsibility three ways and only create confusion. Marshal Stalin had said a ninety-ten agreement for Bulgaria would be acceptable, and, if both sides agreed on that, the rest would be easy. Eden refused because Britain would then be no more than a powerless observer in Bulgaria. Britain wanted more than it had in Rumania. Molotov rejected the American plan, because that would leave the Soviet Union with only a 34 percent voice in Bulgaria during the postwar period. The Russians could accept no less than 90 percent. The Soviets were already making a great concession by not demanding 100 percent. Eden persisted.

Molotov then reopened the entire "percentages" issue. He next proposed a seventy-five-twenty-five division for Bulgaria, Hungary, and Yugoslavia. Eden said that the new proposal was worse than the Russian position of the previous day. Molotov then reverted to a ninety-ten division for Bulgaria and a fifty-fifty division for Yugoslavia. Eden replied that this offer still did not give Britain the voice it wanted in Bulgaria. Molotov finally suggested seventy-five-twenty-five for Hungary and sixty-forty for Yugoslavia, an arrangement that would enable the Soviet Union to accept a seventy-five-twenty-five division for Bulgaria. He could go no further than that. Eden presented a counteroffer: seventy-five-twenty-five for Hungary, eighty-twenty for Bulgaria, and fifty-fifty for Yugoslavia. Molotov returned to his starting point. He could agree to fifty-fifty for Yugoslavia if Bulgaria were ninety-ten. He argued that the British should have as little interest in Bulgaria as in Rumania, since both states were Black Sea, not Mediterranean, countries. The Russians hoped that the British would do for the Soviet Union in the Black Sea what the Russians were prepared to do for Britain in the Mediterranean. Bulgaria, after all, was not Greece, Italy, Spain, or even Yugoslavia.²⁸

Eden remained adamant. Molotov now said that he thought Stalin might

²⁷ "Anglo-Russian Political Conversations," 11, 48. Also see *FRUS*, 1944, 3: 446.

²⁸ "Anglo-Russian Political Conversations," 12-14.

agree to seventy-five–twenty-five for Bulgaria if Britain agreed to sixty–forty for Yugoslavia. At this point Molotov hinted at a partition of Yugoslavia. Since the Soviet Union claimed pre-eminence in the Black Sea and Britain in the Mediterranean, the Russians disclaimed interest in the Yugoslav littoral and were ready to stay in their own “lake.” But Eden favored a joint policy to achieve a united Yugoslavia under a coalition government. Summing up the discussion, Eden stated that the outstanding question pertaining to Bulgaria was the power of the Allied Control Commission after Germany’s surrender. The two sides had already agreed on Moscow as the venue for discussions of the matter and on a joint Soviet-British signature on the Bulgarian armistice.²⁹

The next day, October 11, Molotov offered a compromise formula, which conceded to the British a 20, rather than a 10, percent voice in Bulgaria. Overall, Molotov now proposed an eighty–twenty division for Hungary and Bulgaria and stayed with fifty–fifty for Yugoslavia. But what was the practical difference between 10 and 20 percent? This new division for Hungary and Bulgaria, as Molotov explained it, indicated Soviet acceptance of Eden’s idea that the Allied Control Commission in Bulgaria should act on the instructions of the Soviet High Command but “with participation” of British and American representatives. After some discussion Eden said he thought that the new formulation for Article 18 on the proportioned responsibilities for the Allied Control Commission in Bulgaria would be acceptable. Molotov hoped that the same formula would be applied to Hungary.

And so matters rested. Churchill’s original proposal was applied to Rumania, Greece, and Yugoslavia, but an eighty–twenty division was applied to Bulgaria and Hungary. Stalin and Molotov had extracted from Churchill and Eden a higher percentage for the Soviet Union than the seventy–five–twenty–five division for Bulgaria and the fifty–fifty division for Hungary that Churchill reported in his *Triumph and Tragedy*. The Russians had successfully blocked British and American efforts to gain substantial participation in the Bulgarian armistice and control equal to that of the Soviet Union for the Hungarian armistice. Molotov’s concession assured his Allies nothing more than “participation” in the Allied Control Commissions under the chairmanship and “general direction” of the “Allied (Soviet) High Command.”³⁰ The British seemed to have suffered complete diplomatic defeat in Moscow at the hands of Stalin and Molotov, but this was not entirely the case.

THE QUESTION NATURALLY ARISES, why so much concern with Bulgaria just at this time? And what took the Allies so long to draft an agreed armistice text

²⁹ *Ibid.*, 14–15. Churchill and Stalin agreed on the possibility of an independent Serbia if Yugoslav federation failed, and Stalin acknowledged the primacy of British interests on the Dalmatian coast; Harriman to Roosevelt, Moscow, October 12, 1944, *FRUS*, 1944, 4: 1013–14.

³⁰ “Anglo-Russian Political Conversations,” 16–17, 49–50. For text of the Bulgarian armistice, dated October 28, 1944, signed for the Allied side by F. Tolbukhin, representative of the Soviet High Command, and James Gammell, representative of the Supreme Allied Commander in the Mediterranean, see Rothstein, *Soviet Foreign Policy*, 170–73.

for Bulgaria? Since British and American economic and political interests in that country were minimal, the difficulties at first glance seem puzzling.³¹ But the picture becomes clear when we retrace the discussions among the Big Three over the Bulgarian armistice.

Given Greek, Yugoslav, and Rumanian territory held by Hitler, the Bulgarian dictatorship had joined the Axis and on December 13, 1941, declared war on Britain and the United States. But the bitterly anti-Communist Bulgarian regime dared not declare war on the Soviets—the grandsons of the Russian Orthodox, Slavic brethren who had liberated Bulgaria from the Turks in 1877–78. In the European Advisory Commission the Americans, who still recoiled from Balkan entanglements, let the British take the lead in drafting armistice terms for Hitler's East European satellites. The Soviet Union, still at peace with Bulgaria, either abstained from work on the Bulgarian armistice or let the British and Americans take the lead. The problem of an Allied occupation of Bulgaria proved vexatious. Hitherto the lead in drafting, negotiating, and supervising an armistice had been taken by the Allied occupying power, as in the case of Italy or Rumania, or by the Ally most directly concerned, as in the case of Finland. But Bulgaria was an exception. Here was a German satellite prepared to capitulate to the Allies in the summer of 1944, although no Allied army yet occupied or even approached its territory. Which Ally, then, would prevail in Bulgaria? At that time Britain prevailed by Soviet and American default.

But the Red Army breakthrough in Rumania changed everything. On September 1, 1944, the Red Army reached the Bulgarian border in the south Dobrudja area. On September 5, 1944, the Soviet Union declared war on Bulgaria, because Sofia had refused to declare war on Berlin.³² Moscow had given Washington and London hardly more than an hour's notice of the Soviet declaration of war on Bulgaria.³³ But Soviet troops did not immediately enter Bulgarian territory; Moscow had decided to let the Bulgarians "stew in their juice" a bit. The Bulgarian government severed relations with Germany on September 7 and declared war on Germany the following day, whereupon the Red Army crossed the Bulgarian frontier. On September 9 the Bulgarian government was deposed in favor of a Communist-led Fatherland Front government, which requested that Moscow forward armistice terms; and, after a brief, bloodless war, Soviet troops ceased hostilities in Bulgaria. Ambassador Gusev, the Soviet representative on the European Advisory Commission, asked that discussions of Bulgarian armistice terms be resumed, now with full Soviet participation, and suggested that negotiations with the Bulgarian government ought to be transferred from Cairo to Moscow or to Ankara.³⁴ On September 10 Moscow announced that the Soviet government—

³¹ "Why did Churchill ask for more of a say in Bulgaria than in Rumania? And how does 25 percent say really compare with a 10 percent say? These questions must remain unanswered." Robert Lee Wolff, *The Balkans in Our Time* (Cambridge, 1956), 260.

³² "The Breaking Off of Soviet-Bulgarian Relations" in Rothstein, *Soviet Foreign Policy*, 119.

³³ Hull to Steinhardt (Ankara), Washington, September 9, 1944, *FRUS*, 1944, 3: 407.

³⁴ Gusev to Winant, London, September 9, 1944, *ibid.*, 405–06.

jointly with the governments of Great Britain and the United States—was drawing up terms for a Bulgarian armistice.³⁵ On September 15 Soviet troops entered Sofia.

The surge of Soviet military power in Bulgaria nullified Britain's primacy in that country. Soviet-controlled Bulgaria—abutting Greece, Yugoslavia, and Turkey—posed a threat to Britain's position in the Balkans. The Soviet Union, which had previously sought the evacuation of Bulgarian occupation forces from Thrace and Macedonia in order to aid Marshal Tito, now endeavored to keep them there. Ambassador Gusev tried to persuade the European Advisory Commission that, since the Bulgarian army was now aiding the Red Army against the Germans, the Allies should not require the Bulgarians to withdraw from Grecian Thrace.³⁶

These events found Churchill and Eden preparing for their meeting with Roosevelt at the Second Quebec Conference (September 12–16). Churchill feared that the Soviet declaration of war on Bulgaria might lead to a Soviet invasion of Greece or to Soviet sponsorship of Bulgarian claims to western Thrace. Either could make a Soviet-backed “greater Bulgaria” an Aegean power at the expense of Greece or a threat to the Straits. To secure Greece, Churchill proved willing to recognize Soviet primacy in Bulgaria as well as in Rumania. From Quebec Eden wired to the Foreign Office Britain's willingness to accept a Soviet chairman of the Allied Control Commission for Bulgaria, providing that the British were “full members” and that the Soviet Union recognized the predominant position of Britain in Greece “now and after the actual state of war has ended.”³⁷

When Eden returned to London, the Foreign Office persuaded him that such a concession would undermine British authority in Greece and Turkey. He wired Churchill that Britain could afford to allow the Russians to take the lead in Rumania, but, if Bulgaria—which threatened all of her neighbors—were abandoned to the Russians, Britain's credit would suffer throughout the Balkans. We must, he wrote, “stake our claim to a predominant position in Greece, but we can do this without selling out over Bulgaria.”³⁸ With the Red army in Bulgaria and British troops not yet in Greece, the British were horrified by the prospect of Soviet troops turning south to Athens. On September 21 London therefore instructed Ambassador Clark Kerr in Moscow to inform the Russians that a British force was about to land in Greece and that London hoped “the Soviet Government would not find it necessary to send Russian troops into any part of Greece except in agreement with His Majesty's Government.” Two days later, Deputy Commissar for Foreign Affairs Andrei Vyshinsky said that the Soviet government “confirmed” the

³⁵ For text of the Soviet press bulletin, see *Sovetsko-Bolgarskie otnosheniia, 1944–1948 gg: Dokumenty i materialy* (Moscow, 1969), 14.

³⁶ Phillip Mosely, *The Kremlin and World Politics* (New York, 1960), 230. Mosely, U.S. spokesman on the European Advisory Commission during the talks on Bulgaria, strongly opposed Soviet efforts to leave Bulgarian forces in Greek territories.

³⁷ Barker, *British Policy in South-East Europe*, 143.

³⁸ *Ibid.*; and Woodward, *British Foreign Policy in the Second World War*, 139–40.

agreement of May 1944 about theaters of operations and had no intention of sending forces to Greece.³⁹

Buoyed by this news, the British redoubled efforts to prevent Soviet dictation of Bulgarian armistice terms. Meanwhile, British troops had landed in Greece on October 4 and expected to reach Athens on October 15. Greece was more or less safe. But the Bulgarian armistice discussions remained deadlocked over two issues: withdrawal of Bulgarian troops and civil authorities from occupied Greece and Yugoslavia and the distribution of power within the Allied Control Commission for Bulgaria. Eden dug in his heels to win an equal share in the control of Bulgaria; to give way, he reasoned, would signal abdication of responsibility. Alternatively, the British might sign a separate armistice with Bulgaria. But that would be futile, as the British would have no troops on the spot to enforce terms.⁴⁰ Instead, Churchill and Eden decided to raise the matter with Stalin and Molotov. The British leaders hoped that in Moscow they somehow might salvage something from the usurpers of Britain's primacy in Bulgaria.⁴¹

By the time Churchill and Eden arrived in Moscow on October 9, 1944, the British bargaining position in the Balkans had crumbled. The three-month trial period of the Anglo-Soviet agreement giving Britain the lead in Greece and the Soviet Union the lead in Rumania had expired in September, evidently without renewal by Churchill and Roosevelt at Quebec. Churchill's latest effort to obtain American support for an Allied landing at Istria (Yugoslavia)—for a drive through the Ljubljana Gap to reach Vienna before the Russians—had failed to win wholehearted American support at the Quebec meeting.⁴² Britain's own military power was already stretched so thin that, even positing a sudden military and political collapse of Germany, the British could not move large forces into southeastern Europe. The British chiefs of staff had no intention of stationing forces other than supply guards in Yugoslavia and Albania and anticipated no immediate action in Hungary or Rumania. And for the present the British chiefs also had no intention of sending troops into Bulgaria. Furthermore, on no account would British naval forces enter the Danube except after prior agreement with the Russians.⁴³

Confronted by the overwhelming power of the Red Army everywhere in southeastern Europe except in Greece and Albania, the British asked for an equal share of power within the Allied Control Commission for Bulgaria but were willing to settle for "some share" in that body, their participation to take effect at the end of hostilities in Europe. They regarded securing the withdrawal of Bulgarian occupation troops from Thrace and Macedonia as the

³⁹ Barker, *British Policy in South-East Europe*, 144. Also see page 374, above.

⁴⁰ Barker, *British Policy in South-East Europe*, 144.

⁴¹ This battle promised to be tough, since the Russians backed their demand for terms that ensured Soviet predominance in Bulgaria then and after the war by citing two precedents: Italy, where the Soviet Union had been excluded from all power and responsibility, and Rumania; *ibid.*, 222.

⁴² *FRUS*, 1944, *The Conference at Quebec*, 304–05; and Churchill, *Triumph and Tragedy*, 127, 133, 136.

⁴³ "Memorandum by the British Chiefs of Staff, September 15, 1944," *FRUS*, 1944, *Conference at Quebec*, 439.

most urgent task, especially since the occupation was now under the Communist-led Patriotic Front government. The British, therefore, asked the European Advisory Commission to demand that Bulgaria withdraw its occupation forces within fifteen days of notification as the indispensable prerequisite to the opening of armistice negotiations.⁴⁴ The Soviet representative on the commission reluctantly accepted the proposal on September 20.⁴⁵ But the Allies did not reach agreement on the text of such an ultimatum to Bulgaria until October 11. In the interim the Bulgarians claimed that they had evacuated all of their occupation forces but had left combat units (under Soviet command) for military operations against the Germans in Thrace, Macedonia, and Serbia.⁴⁶ The British feared, however, that this claim was only a subterfuge to leave Bulgarian forces in control of Greek and Yugoslav territories; Britain therefore opposed any semblance of cobelligerency status for Bulgaria. The Russians, for their part, would not accept the British draft of the ultimatum, because it provided an equal voice for each of the three members of the Joint Military Mission that would verify withdrawal of Bulgarian forces.⁴⁷

On October 11, however, everything fell into place. Eden accepted Molotov's demand that the Soviet representative chair the Joint Military Mission. Since on the same day Eden had also accepted Molotov's phrasing of Article 18 on the Allied Control Commission for Bulgaria, the three Allied governments notified the Bulgarian government that the prerequisite for the opening of armistice negotiations was the evacuation of all Bulgarian troops and civil authorities from occupied Greek and Yugoslav territory within fifteen days. The evacuation would be supervised and verified by representatives of the three Allied governments who would "act as a joint Allied Military Mission, with the Soviet representative as Chairman." Bulgaria accepted the condition on October 12.⁴⁸ The agreed terms of the Bulgarian armistice were then sent to the European Advisory Commission in London for coordination with the Americans.

In Moscow, Eden had taken a beating at the hands of Molotov regarding the percentages for Bulgaria. Eden failed to win Molotov's unequivocal agreement to an equal voice for each of the three powers within the Allied Control Commission for Bulgaria during the postwar period. But the British and

⁴⁴ Winant to Hull, London, September 17, 1944, *FRUS*, 1944, 3: 423-24.

⁴⁵ Winant to Hull, London, September 20, 1944, *ibid.*, 431.

⁴⁶ Steinhardt to Hull, Ankara, October 3, 1944, *ibid.*, 442. On October 5, the Fatherland Front of Bulgaria and the Yugoslav National Liberation Committee signed an Agreement on Military Cooperation against Germany, which provided that Bulgarian forces in Yugoslavia would participate with Yugoslavia in joint military operations against the Germans; *Diplomaticheskii slovar*, 1 (Moscow, 1960): 200. Also see "Anglo-Russian Political Conversations," 37.

⁴⁷ For the British draft, see Winant to Hull, London, September 21, 1944, *FRUS*, 1944, 3: 432.

⁴⁸ Incomplete text of the Allied ultimatum published in *Izvestiia*, October 12, 1944. The letter sent by the Soviets contained a secret "explanation," which stated that the evacuation order did not apply to Bulgarian troops conducting operations in Yugoslavia in collaboration with Marshal Tito and the Soviet High Command. For complete text, see Colonel-General Biriuzov to K. Georgiev, chairman of the Council of Ministers of Bulgaria, October 11, 1944, in *Sovetsko-Bolgarskie otnosheniia*, 22. Molotov, however, informed Eden of this reservation on October 16 and Eden approved; "Anglo-Russian Political Conversations," 37.

Americans, by forcing the withdrawal of Bulgarian occupation forces, did succeed in beating back the threat of a "greater Bulgaria," which under Soviet aegis might have challenged British control over Greece.⁴⁹

But complications immediately arose. On October 13, Eden informed the Russians that he "was in trouble over Bulgaria"; the Americans would not accept Article 18 on the Allied Control Commission. Eden tried to harmonize the American and Soviet views. But Molotov rejected this attempt as reneging on an agreement and declared the Anglo-American proposals unacceptable because they would impose harsher terms on Bulgaria than those imposed earlier on Rumania.⁵⁰ For its part, the Department of State was highly dissatisfied with Article 18 of the agreed Anglo-Soviet draft, because it failed to give the United States a voice equal to that of the Soviet Union in Bulgaria after the surrender of Germany. Cordell Hull instructed Ambassador Winant to obtain a more satisfactory version of the article in the European Advisory Commission, although Winant was not to insist to the point of rejecting the armistice agreement. But, if Winant failed, Hull said, "we want it to be clear to all concerned that we may find it necessary at some later date to reopen the question of interpretation of this article."⁵¹ At the next session of the European Advisory Commission, Winant, failing to win adoption of an American draft of Article 18, reserved the right of the U.S. government to reopen the question at a later date. Nevertheless, Winant believed that American efforts "may have slowed down somewhat the tendency to harden Europe into spheres of exclusive influence."⁵² Bulgaria signed the armistice in Moscow on October 28, 1944.

Thus, the stage was set for the endless challenges thrown down by the United States to Soviet primacy in Bulgaria—a harbinger of the Cold War in southeastern Europe. Ambassador Averell Harriman was already asking Washington how far he should go in resisting inevitable Soviet demands that the forthcoming armistice with Hungary assign the United States and Great Britain no voice, or only a token voice, in controlling the execution of the armistice.⁵³

A FINAL QUESTION REMAINS: To what extent was President Roosevelt privy to the Churchill-Stalin understanding? During their discussion of a "percentages" agreement, neither Churchill nor Stalin so much as suggested that their bilateral arrangements were conditional upon American approval. What is more, at no time did either Churchill or Stalin, directly or through Harriman,

⁴⁹ Eden exaggerated when he claimed that "we obtained what we wanted on all points. I should say 90 percent overall." But he could rightly claim victory when he said that the "Soviets will summon the Bulgars out of Greece and Yugoslavia tonight." Eden, *The Reckoning*, 559–60.

⁵⁰ "Anglo-Russian Political Conversations," 19, 48. A convoluted correspondence ensued between Eden and Molotov in Moscow (October 13–17). Eden insisted that each Ally would have an equal voice in Bulgaria after Germany's surrender; Molotov tenaciously denied this interpretation. *Ibid.*, 55–60.

⁵¹ Hull to Winant, Washington, October 21, 1944, *FRUS*, 1944, 3: 469–70.

⁵² Winant to Hull, London, October 22, 1944, *ibid.*, 473–74.

⁵³ Harriman to Hull, Moscow, October 17, 1944, *ibid.*, 460–61.

inform the president or the Department of State of the exact details of the “percentages” agreement. In fact, the British and the Russians went through an elaborate pretense that their informal understanding was not a division of southeastern Europe into spheres of interest, while they were making just such a division. On October 10, for example, Churchill submitted his draft of a joint message, which reported to Roosevelt the gist of the Churchill-Stalin conversation of the previous evening. With Churchill’s approval Stalin crossed out the phrase “having regard to our varying duty towards them” (the Balkan countries), which implied a spheres-of-influence arrangement. Later that day Harriman told Stalin that Roosevelt would be very glad that Stalin had deleted that phrase, because Roosevelt believed that all such questions ought to be dealt with by the three leaders. According to Harriman, Stalin replied that “he was glad to hear this and reaching behind Churchill’s back, shook my hand.”⁵⁴

On the next day Harriman was able to send the president a fairly accurate—though incomplete—account of the arrangement the British and Russians were working out for each country. But there was no mention of a “percentages” agreement.⁵⁵ Harriman asked the president if Secretary Hull was being kept informed. But Hull heard about the “percentages” agreement only in a roundabout way. Winant in London reported that Eden’s cablegrams to the Foreign Office referred to various “percentages” of control, the exact meaning of which was not clear.⁵⁶ Hull cabled Winant two days later to say that he would be grateful for whatever additional information Winant could obtain on the question. In Ankara Ambassador Steinhardt had heard the British ambassador there speak of the “percentages” arrangement in terms of “Anglo-American” to describe the non-Soviet sphere. Hull could not understand how percentages of responsibility might be distributed and had no knowledge of any U.S. participation in such a plan.⁵⁷

Thus, the “percentages” agreement concluded between the British and the Soviets on the Balkans was not left in abeyance pending approval by the United States. Indeed, the United States was not even completely informed of the terms. And the Department of State was left to shift for itself in running down the details.⁵⁸ Roosevelt and Hull showed a puzzling lack of curiosity in

⁵⁴ Harriman to Roosevelt, Moscow, October 10, 1944, *FRUS*, 1944, 4: 1006–07. Gabriel Kolko erroneously took Stalin’s gestures as proof that Stalin had not accepted the “percentages” agreement; *The Politics of War* (New York, 1968), 145–46.

⁵⁵ Harriman to Roosevelt, Moscow, October 11, 1944, *ibid.*, 1009–10. Harriman believed that guilt pangs doubtless led Churchill “not to tell Harriman of his strange bargain with Stalin, except in bits and pieces, spaced over several days”; Harriman and Abel, *Special Envoy*, 357.

⁵⁶ Winant reported that Eden’s cables spoke of 75–25 percent on Hungary and on Rumania, and Eden insisted on joint British-Soviet policy for Yugoslavia, although the Russians referred to 60–40 percent and Eden insisted on a 50–50 percent on Yugoslavia. Winant to Hull, London, October 12, 1944, *FRUS*, 1944, 3: 452.

⁵⁷ Hull to Winant, Washington, October 14, 1944, *ibid.*, 456. Steinhardt reported that the British ambassador to Ankara informed him that Churchill and Stalin had agreed on 80 percent Russian and 20 percent “Anglo-American” representation in Bulgaria and Hungary and 50 percent Russian and 50 percent Anglo-American representation in Yugoslavia; Steinhardt to Hull, Ankara, October 13, 1944, *FRUS*, 1944, 4: 1015.

⁵⁸ Knowledge of the “percentages” arrangement in the Department of State remained scanty and uncertain, and senior officials could only speculate on the details. See, for example, *FRUS*, 1944, 4: 1016–19,

not directly asking the British or the Soviets for details of the "percentages" agreement. Perhaps the president and the secretary of state deliberately chose to ignore the arrangement so long as Churchill and Stalin clearly understood that no agreement on their part was binding on the United States. (Roosevelt had forcefully put that reservation to Stalin on October 4.) Or perhaps the U.S. government did not yet think the problem important enough to warrant a direct inquiry.⁵⁹ Washington, after all, had generally regarded southeastern Europe as an area in which the United States had little interest. American leaders gladly left responsibility for that area to Britain, despite their resentment of Churchill's seeming determination to divert American troops and resources into a Balkan invasion.

At the end of September and in early October 1944, however, a turnabout in United States policy toward southeastern Europe from minimum to great concern was under way.⁶⁰ The rapid advance of the Red Army into southeastern Europe, which made another Big Three conference urgent, found Roosevelt unable for the moment to attend such a conference because of the presidential campaign. The prime minister had suggested to the president that Churchill and Eden should nevertheless proceed immediately to Moscow and try to reach an understanding with Stalin and Molotov on delimitation of spheres of interest in the Balkan area.⁶¹ But on October 3 Harry Hopkins dissuaded Roosevelt from cabling Churchill a good-luck message that might be construed as implying that the United States stood aloof from Balkan affairs or that Churchill should speak for the United States on such matters in Moscow. Advised by Charles Bohlen, Hopkins then persuaded Roosevelt to send Churchill and Stalin separate messages that, in effect, served notice that the United States no longer pursued a passive policy toward southeastern Europe. Thus, on October 4 Roosevelt informed Stalin that he regretted his inability to join his colleagues in Moscow; but the president had to tell the marshal that "in this global war there is literally no question, political or military, in which the United States is not interested." Only the three of them

and 1945, *Conference at Malta and Yalta*, 103-06, 237, 257, 262-64. When two wartime secretaries of state published their memoirs, they provided the public with the first authoritative evidence that an Anglo-Soviet "spheres-of-influence" agreement had been concluded, but they, too, were uncertain of the details. See James Byrnes, *Speaking Frankly* (New York, 1947), 53; and Hull, *Memoirs*, 1458. For a different view, see Lynn Etheridge Davis, *The Cold War Begins: Soviet-American Conflict over Eastern Europe* (Princeton, 1974), 158-59. Churchill did, however, divulge the details to De Gaulle; see Charles De Gaulle, *The Complete War Memoirs* (New York, 1964), 724-25.

⁵⁹ Davis holds that the U.S. government chose to ignore the Anglo-Soviet agreement; *The Cold War Begins*, 159. Herbert Feis concluded that Roosevelt agreed that such an agreement was advisable but wanted to keep the United States aloof from Balkan problems; *Churchill-Roosevelt-Stalin: The War They Waged and the Peace They Sought* (Princeton, 1957), 450-51.

⁶⁰ No American military occupation was intended anywhere in southeastern Europe; *FRUS, Conference at Quebec*, 1944, 212-18. The turnabout was probably precipitated by Harriman's warnings from Moscow that, unless the United States took issue with the present Soviet "strong-arm" policy, there was "every indication the Soviet Union will become a world bully wherever their interests are involved"; Harriman to Harry Hopkins, Moscow, September 10, 1944, *FRUS*, 1944, 4: 989; and Harriman to Hull, Moscow, September 20, 1944, *ibid.*, 992-98. George F. Kennan's memorandum, "Russia Seven Years Later," may also have had an impact; Moscow, September, 1944, *ibid.*, 902-14.

⁶¹ Robert Sherwood, *Roosevelt and Hopkins* (New York, 1948), 832; and Churchill, *Triumph and Tragedy*, 181, 186-88.

could find the solution to questions still unsolved. Roosevelt suggested that Stalin and Churchill allow Ambassador Harriman to act as the president's observer at the Churchill-Stalin meetings—without the power to commit the United States on any major issue. This message disabused Stalin of the assumption that Churchill would be empowered to speak for Roosevelt in Moscow.⁶² Although Roosevelt's turnabout struck a final blow at Churchill's bargaining position, the prime minister was not deflected from his determination to reach an agreement on spheres of influence with Stalin.⁶³

IF, THEN, WE WANTED TO FIX AN EXACT DATE for the beginning of the Cold War in southeastern Europe, that date would be October 4, 1944. For on that day President Roosevelt informed Stalin that the president reserved for the United States the right to nothing less than a voice at least equal to that of each of the other Big Three powers in arriving at and executing decisions on all international problems—including those in southeastern Europe—while denying the Soviet Union the same right in the Western spheres—in Italy, for example. In short, the United States now embarked on a policy that clearly denied to the Soviet Union status equal to that of the United States. For Washington would not accede to Moscow's exercise of undivided control, in the name of the Big Three, over the future armistices for Bulgaria and for Hungary, although each draft armistice followed the Italian precedent with regard to the Allied Control Commission. Since the Russians regarded their hegemony over the southeastern European approaches to the Soviet Union as crucial to Soviet security, Stalin's ire over Roosevelt's pretensions can be imagined.⁶⁴ Whether Roosevelt's assertion of boundless American interests is termed "internationalism," "globalism," or "imperialism," this turnabout certainly set American policy concerning southeastern Europe on a new course.

As for Britain, Churchill and Eden had maintained since 1941 that it was impractical to assume that Great Britain and the United States could compel a victorious Soviet Union to accept frontiers shallower than those it had had in 1940. London contended that the surest way for Great Britain and the United States to check Soviet expansion beyond those borders in Europe was to reach a wartime agreement that would tie Moscow to the western frontiers Stalin had claimed since 1941. A similar rationale governed the subsequent British

⁶² Sherwood, *Roosevelt and Hopkins*, 833–34; Charles Bohlen, *Witness to History* (New York, 1973), 162–63; Roosevelt to Harriman, Washington, October, 1944, *FRUS*, 1945, *Malta and Yalta*, 6–7; and Stalin to Roosevelt, Moscow, October 8, 1944, *Correspondence between the Chairman of the Council of Ministers of the U.S.S.R. and the Presidents of the U.S.A. and the Prime Ministers of Great Britain during the Great Patriotic War of 1941–1945*, 2 (Moscow, 1957): 162–63.

⁶³ The prime minister informed the president that he would be glad to have Harriman sit in at all principal conferences but he hoped that Roosevelt would not preclude private meetings between himself and Stalin or Eden and Molotov; Churchill, *Triumph and Tragedy*, 190–91. The meeting of October 9, when the "percentages" agreement was concluded, was just such a tête-à-tête without Harriman's presence.

⁶⁴ On October 9 Stalin told Churchill that he did not like Roosevelt's message of October 4, because "it seemed to demand too many rights for the United States leaving too little for the Soviet Union and Great Britain, who, after all, had a treaty of common assistance"; "Anglo-Russian Political Conversations," 5.

decision to divide southeastern Europe into Anglo-Soviet spheres of interest. But the U.S. government opposed this approach, contending that such war-time agreements would feed—not curb—Soviet expansion,⁶⁵ violate the Atlantic Charter, and increase the danger of war between Britain and Russia. Failing to win full American support for his military solution—an invasion of the Balkans—or for his political solution—a division of southeastern Europe into Anglo-Soviet spheres of interest—Churchill tried to go it alone in treating with Stalin. Hence, the "percentages" agreement.

The agreement was put to the test almost immediately. And for a time it worked. Churchill ordered the British army to suppress leftist armed resistance to the royal house in Athens. He covered himself against political attacks from the Left and from the East by charging in the House of Commons that the Greek rebels were the worst kind—"Trotskyists."⁶⁶ But he did not have to worry about any Soviet disapproval of British pacification of the Greek Left; Stalin honored his understanding with Churchill and remained silent on Greece.⁶⁷ When Stalin intervened in Rumania in February 1945 to install a Communist-dominated government there, Churchill kept his peace.⁶⁸ To the displeasure of both Churchill and Stalin, the U.S. government and sections of the American press strongly condemned British action in Greece and then Soviet action in Rumania. Stalin was nettled by American attacks on Soviet conduct in Rumania and Bulgaria, despite Soviet forbearance in Greek affairs.⁶⁹ Churchill's exasperation with Americans, who accused British imperialists of playing "power politics" in Greece, erupted in a caustic but prescient rebuff. Firing off a *tu quoque* at his American critics, he said,

What are power politics? . . . Is having a Navy twice as big as any other Navy in the world power politics? Is having the largest Air Force in the world, with bases in every part of the world power politics? Is having all the gold in the world power politics? If so, we are certainly not guilty of these offences, I am sorry to say. They are luxuries that have passed away from us.⁷⁰

The "percentages" agreement worked until Britain proved too weak to sustain its side of the bargain. The United States inserted itself increasingly into Balkan affairs and finally, in March 1947, replaced faltering British power in that area. Few would now say that America's enormous power inspired a wiser, more effective policy toward southeastern Europe than Churchill's brand of "power politics" expressed in the "percentages" agreement.

⁶⁵ For a succinct account of this divergence of views, see Eden, *The Reckoning*, 370–71.

⁶⁶ Winston Churchill, Speech to the House of Commons, January 18, 1945, in House of Commons, *Parliamentary Debates*, 5th ser., 1944–1945, 407: col. 403; and Churchill, *Triumph and Tragedy*, 270.

⁶⁷ Churchill, *Triumph and Tragedy*, 246, 252.

⁶⁸ Churchill to Roosevelt, March 8, 1945, in F. Loewenheim et al., eds., *Roosevelt and Churchill: Their Secret Wartime Correspondence* (New York, 1976), 660–62; and Eden, *The Reckoning*, 604–05.

⁶⁹ Churchill, *Triumph and Tragedy*, 543.

⁷⁰ Churchill, Speech to the House of Commons, January 18, 1945, in *Parliamentary Debates*, 1944–1945, 407: cols. 423–26.

AHR Forum

The Rosenberg Case Revisited: The Greenglass Testimony and the Protection of Atomic Secrets

ROGER M. ANDERS

IN THE OCTOBER ISSUE of the *American Historical Review* Michael Parrish described how the case of the *United States v. Julius Rosenberg et al.* was handled by the federal judiciary, particularly by the Supreme Court. The topic did not require Parrish to pass judgment on the guilt or innocence of Julius and Ethel Rosenberg—a question that remains, after more than a quarter of a century, the subject of heated debate. The presumption of their guilt rests largely on the testimony of Ethel Rosenberg's brother, David Greenglass. Documents recently declassified show that the Atomic Energy Commission judged that the statements made by Greenglass to the Federal Bureau of Investigation about the atomic bomb were reasonably accurate. The prospect that he would repeat those statements in open court in the Rosenberg trial created a major security problem for the commission. It feared that the questioning of Greenglass and of expert witnesses would reveal

The author wishes to express his appreciation to James G. Beckerley and William Denson, each of whom generously shared his experiences in the Rosenberg case with the author. Naturally neither is responsible in any way for any of the opinions or conclusions expressed in this paper, nor are any of the opinions or conclusions in this paper those of the Department of Energy. All Atomic Energy Commission (AEC) documents cited in the footnotes have been rendered unclassified and are available to the public. Some of the documents have words or phrases deleted to make them unclassified. Most of the sources used are photocopies of documents consolidated into a single file in response to a query to the Historian's Office; these staff papers do not, therefore, carry AEC file or record group numbers. Copies of any of the Commission documents can be obtained from the Historian's Office, U.S. Department of Energy, Washington, D.C.

The *AHR Forum* is a new feature that will reappear in future issues of the *Review* whenever the opportunity arises. It will contain articles that differ in one way or another from those traditionally accepted by the editors. This article, for example, is a study based on recently declassified official materials and continues a discussion begun in the October issue of last year: Michael E. Parrish, "Cold War Justice: The Supreme Court and the Rosenbergs," *AHR*, 82 (1977): 805–42. Later issues will contain provocative articles followed by brief responses from scholars who have contrasting views. The character of the *AHR Forum* will be determined, in other words, by the kinds of articles we receive. Contributions are welcome. THE EDITOR

technological secrets about the atomic bomb that were even more sensitive than those Greenglass claimed to have given the Russians.

By the testimony presented by Greenglass himself in the United States District Court for the Southern District of New York in March 1951, Julius Rosenberg recruited David Greenglass to steal secrets of the World War II atomic bomb project for the Soviet Union. While a soldier in the United States Army, Greenglass had been assigned as a machinist to the laboratory at Los Alamos, New Mexico, which assembled the atomic bombs dropped on Hiroshima and Nagasaki. On the witness stand he stated that in November 1944 his wife Ruth, while in New Mexico on a visit, had conveyed to him a request from Julius Rosenberg. Julius, who had learned of David's work on the atomic bomb, asked him to relay information on the project to the Russian government in order to help a deserving ally of the United States. Greenglass stated that, after some hesitation, he sent Ruth back to Rosenberg with a description of the physical plant at Los Alamos and the names of some scientists working on the project. According to his further testimony Greenglass, while in New York on leave in January 1945, gave Rosenberg another list of scientists at Los Alamos, a list of people at Los Alamos who Greenglass thought might be sympathetic to the Russian cause, and, most important of all, a description and sketches of lens molds being used in experiments on the detonation of the bomb. On the same furlough he answered questions from an unknown Russian about lens molds.

At Albuquerque in June 1945 he was visited by Harry Gold, who had come from New York to pick up other atomic bomb secrets from scientist Klaus Fuchs, a German émigré and British citizen. Greenglass stated that he gave Gold a more detailed description of experiments using implosion to detonate the atomic bomb, more sketches of lens molds, and another list of prospective recruits for Soviet espionage. On leave again in New York in September 1945 Greenglass, according to his own testimony, gave Julius Rosenberg further lists of Los Alamos scientists and potential recruits for espionage, a detailed description of the atomic bomb dropped on Nagasaki, and a drawing of a cross section of that bomb. Finally, Greenglass told the court that in February 1946 he had ended his career as a spy (against the wishes of Julius Rosenberg) by leaving Los Alamos and the army.¹

FOR DAVID GREENGLASS THE PATH TO THE WITNESS STAND began in London, where on February 2, 1950, Klaus Fuchs, who had also worked at Los Alamos, confessed to British investigators that he had betrayed secrets of the atomic bomb project to Russia. As soon as American authorities became aware of Fuchs's confession, the FBI launched an intensive investigation on this side of the Atlantic for his accomplices. On May 24 newspapers announced that the chemist Harry Gold had been arrested on the basis of information provided by Fuchs. On June 17 the American public learned that David Greenglass had

¹ Joint Committee on Atomic Energy (hereafter JCAE), *Soviet Atomic Espionage* (Washington, 1951), 69-75, 79-83, 93-98.

been arrested for giving atomic secrets to Harry Gold. On July 17 Julius Rosenberg was arrested and charged with espionage, and one month later Ethel Rosenberg was as well. The case that the Department of Justice later called "the most serious espionage matter in the history of the nation" had begun.²

On June 14, 1950, the Atomic Energy Commission entered the case upon routine request by transmitting to the FBI David Greenglass's security file. Two days later the FBI informed the commission's Division of Security that Greenglass had admitted giving Harry Gold a sketch of a high explosive lens mold used in the atomic bomb. The FBI wanted to know whether such a sketch was considered classified information. The Division of Security replied that it was and quickly set out to discover what David Greenglass had done while working in atomic energy programs. The division's investigation disclosed that from August 7, 1944, to February 20, 1946, Greenglass had been assigned to the Los Alamos facility, where he had worked as a machinist under the direction of research teams studying implosion as a means for triggering an atomic explosion. (Implosion entailed blowing a piece of plutonium in on itself to form a critical mass capable of causing an atomic explosion. Eventually, the Los Alamos scientists perfected implosion and used it to set off the atomic bomb dropped on Nagasaki.) Knowing how Greenglass was employed at Los Alamos, the Division of Security stood ready to give technical aid to the FBI, which, meanwhile, was questioning Greenglass.³

David Greenglass did not immediately disclose all of the atomic bomb data he later claimed he had passed to the Russians. During the summer of 1950 he gradually told more and more about his activities. Soon after his arrest he redrew the sketch he said he had given to Gold. Within two weeks he admitted to giving Gold the lists of scientists working at Los Alamos. By July 17 he confessed to providing Julius Rosenberg with more lens mold sketches and descriptions of implosion experiments.⁴ By July 19 he revealed that he had been able to piece together a description of the entire Nagasaki bomb and passed it on to Julius Rosenberg. As Greenglass recalled it in 1950, the description of the bomb was as follows:

The A bomb is made up of 36 pentagonal shaped molds. When all set together the molds formed a sphere made of high explosives with balls of plutonium in the center. Inside the plutonium is a sphere of beryllium. A plastic shield is in between the plutonium and the high explosives. The high explosive has two detonators for each high explosive segment. The detonators were connected to a number of condensers possibly about 36 or 72. The condensers are also called capacitors. The number of detonators was twice the number of molds and the number of condensers were equal in

² Washington *Evening Star*, May 24, 1950; New York *Herald Tribune*, June 17, 1950; Philadelphia *Inquirer*, June 17, 1950; New York *Times*, July 18, August 12, 1950; and Peyton Ford to Gordon E. Dean, February 28, 1951, enclosure A in AEC 403/6.

³ John L. O'Gara to Edwin A. Eisenhart, June 8, 1950; C. Arthur Rolander to the File, June 22, 1950. For a description of the development of both the Hiroshima and Nagasaki bombs at Los Alamos, see Richard G. Hewlett and Oscar E. Anderson, Jr., *A History of the U.S. Atomic Energy Commission*, vol. 1: *The New World, 1939-1946* (University Park, Pa., 1962), 226-54, 310-20, 374-80.

⁴ Francis Hammack to J. Edgar Hoover, August 2, 1950.

number to the number of detonators. The beryllium had a hollow center. The beryllium had a number of cone shaped holes in it, the apex of each cone being toward the periphery of the beryllium. The beryllium was gold plated which prevented it from emitting neutrons. The detonation of the high explosives causes the plutonium to be crushed or imploded and, therefore, increases the number of plutonium neutrons which are free. The beryllium also becomes crushed and the beryllium neutrons are jetted out through the cones into the mass of the plutonium which sets off the chain reaction of the plutonium which results in the atomic explosion.⁵

As the atomic spy case expanded, so did the commission's involvement in it. On June 21, the FBI asked the commission to provide a witness who could describe to a grand jury in Albuquerque, New Mexico the nature of Greenglass's work on the atomic bomb and its sensitivity. By June 30 FBI reports on Greenglass were being circulated among the commissioners. As Greenglass revealed his activities, the FBI asked the commission whether the information obtained about the atomic bomb was or had been classified. When investigators discovered a lucite disk in the Greenglass apartment, the FBI asked whether the disk, too, was classified. Replying to FBI queries in early August, the Division of Security pointed out that by 1950 the physical details about the Los Alamos project, such as the size of the wartime effort and the number of people working on it, had been declassified. No longer secret were the names of important scientists associated with the project. Also unclassified—although without any accompanying description of how it functioned in the atomic bomb—was the lucite disk. But the sketches of high explosive lens molds, Greenglass's work on implosion experiments, and the description of the atomic bomb were still classified; the lens molds were the least sensitive and the description of the bomb, the most. The description of the bomb was so sensitive, in fact, that the staff did not think it could be declassified.⁶

In 1950 David Greenglass was trying to remember events of five years past. Whether his memory was fogged by the passage of time or deceived by the lack of more formal technical training, his information contained inaccuracies about the atomic bomb project. He got most of his information, except for the description of the atomic bomb, reasonably correct. But in the description of the bomb, as the commission knew, Greenglass was somewhat confused about the number of condensers and detonators. His description of the beryllium

⁵ Extract of Greenglass Confession, typed July 19, 1950. Greenglass gave this same basic description of the atomic bomb on the witness stand. The thirty-six lens molds, seventy-two condensers, and the shells of plastic, plutonium, and beryllium remained. (He had apparently settled on seventy-two as the number of condensers.) Greenglass did add a detail—albeit inaccurate—that further described the plastic shield as a barium plastic shield. Actually, it was a boron plastic shield. Washington *Evening Star*, March 13, 1951; New York *Times*, March 13, 1951; and Arnold Kramish to the File, app. C in John A. Waters to the AEC, February 7, 1951, AEC 403/1. Both John Wexley and Walter and Miriam Schneir state that Greenglass fabricated his testimony with the help of Harry Gold. See Wexley, *The Judgment of Julius and Ethel Rosenberg* (New York, 1955), 89, 202–03, 205–06, 434–35; and Schneir and Schneir, *Invitation to an Inquest* (New York, 1965), 416–17. There is no indication of such fabrication in Atomic Energy Commission documents. An examination of the Statement of Harry Gold before Myles J. Lane and James G. Beckerley, March 12, 1951, and of Gold's testimony at the Rosenberg trial indicates that Gold knew very little about the technology of the atomic bomb: see JCAE, *Soviet Atomic Espionage*, 145–62.

⁶ Rolander to the File, June 22, 1950; Hammack to Carleton Shugg, June 30, 1950; Hammack to Hoover, August 2, 1950; and Waters to the AEC, February 1, 1951, AEC 403.

sphere, moreover, was incomplete, because he was not aware that it contained polonium. On August 5 the staff of the Division of Security informed the FBI that some parts of his technological information were inaccurate and for the first time offered to point out which details were wrong.⁷

By early August the commission had begun to realize that Greenglass's knowledge of the bomb was sufficiently accurate and detailed that a serious security problem would arise if he had to reveal in court all that he knew. Even more worrisome was what other witnesses would have to say in court. For Greenglass's testimony to stand up, experts familiar with the design of atomic weapons would have to attest to the general, if not the specific, accuracy of his story. And should a technical expert reveal in his testimony one detail of the atomic bomb other than what had been specifically declassified by the commission, he could provide Russia with other secrets of bomb design. Added to what Greenglass and Fuchs had already given to the Russians, such disclosure could be a serious blow to America's superior technological position in the nuclear arms race. To prevent accidental disclosures, the commission staff thought U.S. attorneys should be consulted as to what information might be brought out in the direct questioning and cross-examination of technical experts. Once this was determined, the commission could consider whether or not that information should be released in court. In addition, the staff thought that someone in the Justice Department should discuss with the judge who would preside over the Rosenberg trial the problem of revealing information about the atomic bomb in court.⁸

On October 10, 1950, David Greenglass, Julius Rosenberg, and Ethel Rosenberg were indicted on charges of transmitting information relating to the country's defense to the Soviet Union. Now that the Rosenberg case was going to court, the question of how much of the Greenglass data would or should be made public became a pressing problem for the commission to resolve. In a report to the commission, John A. Waters, director of the Division of Security, outlined the problem. After describing the defendants' alleged crimes, Waters explained why the Justice Department wanted Greenglass to testify. The department, Waters declared, believed that Julius Rosenberg was a "paymaster" and "recruiter" for a nationwide espionage ring, some of whose members might still be stealing atomic or other defense secrets. In order to apprehend the rest of the ring, the Justice Department had to put pressure on Julius Rosenberg to name his confederates. To crack him, the department wanted to confront Julius with the death penalty and Ethel with a long prison sentence. To show the real significance of the data the Rosenbergs were

⁷ Rolander to the File, July 31, 1950; Hammack to Hoover, August 2, 1950; Waters to the AEC, February 7, 1951, AEC 403/1; and Kramish to the File, app. C in Waters to the AEC, February 7, 1951, AEC 403/1. Commission records indicate that Arthur Rolander asked the FBI which technical details it wanted verified, though the documents do not reveal what details were actually verified. It is clear, however, that Greenglass never learned of the inaccuracy of some of his information; he took the same basic description of the atomic bomb, including errors, that he gave the FBI in July 1950 to the witness stand in March 1951. Rolander to the File, July 31, 1950; and n. 5, above.

⁸ Rolander to the File, July 31, 1950; Hammack to Hoover, August 2, 1950; James G. Beckerley to Rolander, August 29, 1950; and Waters to the AEC, February 7, 1951, AEC 403/1.

accused of giving to the Russians, the department had to use as much of Greenglass's information as possible. But his proposed testimony contained substantial information that was still secret—sketches of lens molds, descriptions of implosion experiments, and the general design of the Nagasaki bomb and how it worked as well as a description of certain experiments called “levitation” that had also been conducted at Los Alamos. There was danger, furthermore, that technical experts called to verify the general accuracy of Greenglass's statements might inadvertently reveal additional details about the atomic bomb that were still classified. Among the expert witnesses the Justice Department might call was Walter F. Koski, the scientist who developed the experimental implosion arrangements that Greenglass had compromised. If the commission allowed his testimony, Koski could relate how lenses were arranged to achieve the symmetrical detonation waves focused inward toward a specific point—sensitive information also still classified as secret.

Waters concluded that the commission had four options: (1) withhold all classified information, (2) release all classified information, (3) limit disclosures to data about implosion experiments at Los Alamos, or (4) reveal everything save for the levitation experiments. Waters favored the last alternative. Since no one was sure Greenglass had told the FBI everything he had learned about the technology of the atomic bomb, Waters promised to make more specific recommendations after members of the commission staff had had a chance to question him and to determine exactly how much he knew about the atomic bomb.⁹

On February 2, 1951, Dr. James G. Beckerley, director of the Division of Classification, guided Assistant U.S. Attorney Myles J. Lane, who had been tapped to prosecute the Rosenbergs, in a six-hour interrogation of David Greenglass. Beckerley was interested both in Greenglass's technical knowledge and in how he got it. After several questions aimed at determining the amount of information to which Greenglass had gained access, Beckerley went to the description of the implosion bomb.¹⁰ In going over it sentence by sentence, Beckerley discovered that Greenglass had used the following methods to obtain his information:

<i>Item</i>	<i>Source of Information</i>
a. . . . 36 pentagonal shaped molds.	Worked on individual experimental molds. Assumed they were used in weapon. Number derived from detonator information. (see below.)
b. . . . molds form a sphere made of HE, with ball of plutonium in center.	Was asked to section imploded natural uranium spheres. Found out in conversation that plutonium would be used and that the uranium was intended to simulate plutonium.

⁹ Waters to the AEC, February 1, 1951, AEC 403.

¹⁰ JCAE, Transcript of Hearing, February 8, 1951; and Beckerley and Kramish to the Files, app. A in Waters to the AEC, February 7, 1951, AEC 403/1.

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| c. Inside . . . a sphere of beryllium. | Was asked for assistance by someone who was machining the beryllium. From shape and size (about 2 inches) he assumed it was to be in center of weapon. Saw that sphere was hollow. |
| d. A barium plastic shield | Saw one in Sigma building. |
| e. . . . two detonators | Heard that there was an extra one for safety. Assumed use in weapon. |
| f. . . . number of condensers, . . . 36 or 72. | Saw Trinity unit (didn't know name of it) during visit to C Building. Counted condensers. Was told in conversations of intended use. Noted that condensers were mounted on metal plate. |
| g. . . . number of detonators is twice. . . . | Assumed from knowledge of total number of condensers. |
| h. The beryllium . . . emitting neutrons. | Saw the unit, including conical projections. (Proud of his knowledge of "Munroe Effect." Was told about "shaped charges" in conventional ordnance when previously at Aberdeen). |
| i. . . . neutrons are jetted out. . . . | Deduced. ¹¹ |

As the session went on, Beckerley concluded that Greenglass had passed on a great deal of information via the sketches and written descriptions—all of it pieced together from details gathered at random and supplemented by sound guesses. Greenglass had even drawn a sketch of the levitation experiment for the Russians, but fortunately he did not know very much about the principle involved. Beckerley decided that Greenglass could not compromise any more details about the atomic bomb: he had told the FBI everything he could remember. His technical competence was that of a machine shop worker and he might become confused under expert cross-examination. But Beckerley felt that Greenglass was telling the truth since no inconsistencies could be detected in his story.¹²

Knowing what Greenglass could reveal about the atomic bomb, Waters was now ready to recommend a course of action to the commission. In a second report he concluded that the commission should allow Greenglass to reveal a

¹¹ Beckerley and Kramish to the File, app. A in Waters to the AEC, February 7, 1951, AEC 403/1. Trinity Unit was a code name for the atomic bomb exploded at Alamogordo, New Mexico on July 16, 1945. "Monroe effect" refers to Charles E. Monroe's discovery in 1888 that concave-shaped explosive charges would act with unusual force at the focal point of the shock waves. Walter and Miriam Schneir feel that Greenglass pieced his description of the bomb together after the bombings of Hiroshima and Nagasaki from a combination of gossip, newspaper reports, and the Smyth report; see *Invitation to an Inquest*, 274. But, in fact, Greenglass got his information during the construction of the Nagasaki bomb and gathered it by observation, inquiry, and deduction.

¹² Beckerley and Kramish to the File, app. A in Waters to the AEC, February 7, 1951, AEC 403/1.

good deal of the classified information in court despite the danger that he might disclose data not already compromised. The potential benefits seemed to outweigh the risks. It appeared certain that Greenglass's purloined data had been conveyed to the Russians and that the damage done to America's nuclear lead had already been absorbed. The information was also rather general: it did not reveal any specific dimensions of the bomb. And severe sentences seemed necessary if atomic and defense projects were to be protected from further activities of the spy ring to which Rosenberg was believed to have belonged. It appeared, in fact, that Ethel was weakening and that other members of the alleged ring might soon be named.

Waters suggested various ways to protect and limit the technological data revealed in court. Instead of declassifying the Greenglass information as such, the commission could declassify only those details from it that came out in open court. The prosecution could shape Greenglass's testimony, furthermore, so that all of the details would appear to pertain only to the 1945 atomic bomb. This tactic would avoid compromising the technology used in atomic weapons developed later. To insure that no witnesses, whether for the prosecution or for the defense, revealed any further details about the bomb, Waters recommended that a commission employee be present at the trial ready to intervene should the testimony begin to reveal unauthorized facts.

Waters recognized that Greenglass's testimony would quickly become the subject of intense speculation in the press. Employees of the commission would surely be asked for their opinions and might, in replying, unwittingly reveal further secrets about the bomb. Since this could be as damaging as any slip in court, Waters recommended that a directive be sent to all employees cautioning them against public speculation about the technological data in the Greenglass testimony. Waters hoped that all of these measures—*de facto* declassification of the testimony, limitation of details to 1945 weapons, the presence of an agent in the courtroom, and the cautionary directive—would protect those atomic secrets not already compromised by Greenglass. But, before a final decision was reached on what could be revealed at the trial, Waters concluded, the commission must find out from the Justice Department whether all of Greenglass's data about the atomic bomb had to be released and what the likelihood was that the weaponry information in it had reached Russia.¹³

THE COMMISSION TO WHICH WATERS PRESENTED HIS REPORT was composed of Commissioners Sumner T. Pike, Henry D. Smyth, Thomas E. Murray, T. Keith Glennan, and Chairman Gordon E. Dean. Although each member shared equally the responsibility for making decisions on atomic energy programs, Dean quickly emerged as the commissioner most concerned with the Rosenberg case. Forty-six years old in 1951, Dean was an alumnus of the University of Southern California law school. He had learned the ways of

¹³ Waters to the AEC, February 7, 1951, AEC 403/1.

Washington first in the Justice Department during the New Deal and then in a private law firm. After World War II he had joined the faculty of the University of Southern California. Dean returned to Washington in 1949 as a member of the Atomic Energy Commission, and in July 1950 President Truman appointed him chairman.¹⁴

After Waters presented his reports on February 7, 1951, the commission did not immediately reach a final decision on the Greenglass testimony. Previously, Dean had decided to ask the Department of Defense about the wisdom of releasing the testimony. Now the commission agreed to Waters's recommendation that the Justice Department be consulted. Always sensitive to Washington's political environment, Dean also decided to follow another Waters's suggestion—to explain the case to the commission's congressional watchdog, the Joint Committee on Atomic Energy.¹⁵

On February 7 Dean telephoned James M. McInerney, head of the Justice Department's Criminal Division. Dean asked for an expert to help present the Rosenberg case to the joint committee, and McInerney, unable to attend himself, promised to send Myles Lane. McInerney confided to Dean that the judge was ready to impose the death sentence on Julius Rosenberg if the evidence warranted it, but he could not promise that the accused would buckle under the threat, thereby justifying the risk to be taken in releasing details of the Greenglass testimony.¹⁶

Armed with the consent of the Department of Defense for the release of the Greenglass testimony, Gordon Dean and Myles Lane presented the Rosenberg case to the joint committee on February 8, 1951. Lane described Julius Rosenberg's alleged role as leader of an espionage ring and outlined the information that had been given to the Russians. He made it clear that the prosecutors had to have the testimony of David Greenglass to justify the death penalty. Lane believed that Julius Rosenberg was so tough that only the death penalty would force him to talk. If he were given the death sentence, Lane added, the case would go all the way to the Supreme Court. As strong a case as possible had to be made against the Rosenbergs, although Lane admitted that the case against Ethel was weak.¹⁷

Gordon Dean added that the commission believed Greenglass was telling the truth. Since his information was already in the hands of the Russians and since much of what would come out in court would be rather general in nature, the commission was willing to let him testify. The commission did want to protect as many specific details as possible about the "initiator," a name given the device which began the chain reaction in the atomic explosion, and about the levitation principle. Since the commission was still not completely convinced that a clever cross-examination of Greenglass would not reveal more secrets, Dean assured the committee that the commission would have an observer (Beckerley had been chosen) at the trial, who would prevent

¹⁴ Anna Rothe, ed., *Current Biography: Who's News and Why, 1950* (New York, 1951), 113–15.

¹⁵ Waters to the AEC, February 7, 1951, AEC 403/1; Commission Meeting 523, February 7, 1951; Robert Le Baron to Dean, February 8, 1951, AEC 403/2; and JCAE, Transcript of Hearing, February 8, 1951.

¹⁶ Dean Diary (largely composed of notes of Dean's telephone conversations), February 7, 1951.

¹⁷ Le Baron to Dean, February 8, 1951, AEC 403/2; and JCAE, Transcript of Hearing, February 8, 1951.

unauthorized disclosure of classified data. He reminded the committee that the Russian atomic bomb exploded in 1949 had used the implosion principle. Dean offered this as further evidence that Greenglass had transmitted the bomb secrets to Russia through Julius Rosenberg and Harry Gold.

The joint committee was also concerned that the Rosenberg defense lawyers might use their cross-examination of Greenglass and other prosecution witnesses to extract more details about the bomb that were unknown as yet to the Russians. Dean assured the committee that the cross-examination of government witnesses by the defense could be limited by what prosecutors asked those witnesses during direct examination. Testimony could also be limited, moreover, to the 1945 weapon, and many specific technological details could be kept out of the Greenglass testimony. The Joint Committee on Atomic Energy speculated that the Rosenberg lawyers would subpoena prominent American nuclear physicists as defense witnesses and ask them questions about the 1945 bomb or more recent atomic weapons. When pressed by the committee to say whether defense witnesses could use the Atomic Energy Act as a shield to keep from answering such questions, Myles Lane had to confess that he did not know. To the committee as well the possible gains outweighed the risks. The Rosenbergs had to be prosecuted. The committee was convinced that the commission and the Department of Justice were taking adequate precautions to protect information not compromised by Greenglass. The committee urged both agencies to cooperate and to find a way to get as much of Greenglass's atomic bomb data as possible into the court record in order to convict the Rosenbergs.¹⁸

Having consulted the Department of Defense, Department of Justice, and the Joint Committee on Atomic Energy, the commission met on February 13 for a final decision on whether to release the Greenglass testimony. The commissioners agreed to let David Greenglass tell most of his story and followed Waters's recommendation not to declassify the testimony formally. Use of the data in the courtroom was to be a *de facto* declassification of whatever Greenglass said on the witness stand. Once out, those details could never be reclassified. All testimony about the atomic bomb was to be limited to the 1945 weapon in order to protect more current weapons technology. Since the information about levitation was the most sensitive part of the Greenglass confession, the commission hoped that it would not be used in court. The commission, however, decided to allow its use if the Justice Department had to have it to get a conviction. Speaking for the Justice Department later, Deputy Attorney General Peyton Ford assured the commission that the prosecutors would try to observe these precautions.¹⁹ And, in order to keep its own personnel from speculating about the technological data

¹⁸ JCAE, Transcript of Hearing, February 8, 1951.

¹⁹ Commission Meeting 524, February 13, 1951; Draft Letter to the attorney general from the chairman, AEC, February 12, 1951, AEC 403/3; Dean to J. Howard McGrath, February 21, 1951; and Ford to Dean, February 28, 1951, enclosure A in AEC 403/6. Thus, the commission did not try ineffectually to stuff the genie of released information back into the classified bottle, as Louis Nizer states; see his *The Implosion Conspiracy* (Greenwich, Conn., 1974), 104. The commission's general counsel later concluded that the testimony could not be reclassified. Charles L. Marshall's report to Alvin R. Leudecke, September 17, 1959, AEC 403/8.

which did come out in court, the commission agreed to issue the directive Waters had recommended.

ON MARCH 2, THE COMMISSION LEARNED from its liaison men in New York, William Denson and C. Arthur Rolander, Jr., that Irving H. Saypol, the U.S. attorney for the Southern District of New York, rather than Myles Lane, would prosecute the Rosenbergs. Saypol was apparently not going to be as careful as Myles Lane had promised to be about releasing weapons technology in court; Saypol could, therefore, seriously endanger some post-1945 advances. Saypol's appearance in the case upset the compromise reached with Lane and renewed the commission's concerns. That same day Dean explained the commission's position to Peyton Ford and received his unqualified support. On March 5, the day before the trial began, Denson and Rolander met Saypol in New York and were disturbed by the desire of the new prosecutor and his young assistants to conduct wide-ranging direct examinations not only of David Greenglass but also of Walter F. Koski and John A. Derry (the technical experts chosen to support Greenglass's story). They alerted Gordon Dean, who called Peyton Ford again on March 7 to point out that the questions Saypol intended to ask Koski and Derry would give Rosenbergs' defense counsels the opportunity to probe for atomic secrets during cross-examination. Dean wanted a copy of the questions they intended to ask so that he could eliminate those that were potentially harmful. Ford quickly assented and offered the questions for Greenglass as well.²⁰

On March 8, James McInerney assured Dean that Greenglass, Koski, and Derry would be handled with care. Since Koski was scheduled to testify later that day, his questions could only be cleared with the commission staff in New York. The questions for Derry would be given to the commission over the weekend. Later that day Dean received a call from Republican Senator Bourke Hickenlooper of Iowa, one of the members of the joint committee. Having recently learned of the amount of information Saypol wanted to bring out at the trial, Hickenlooper was very disturbed and urged Dean to go all the way to the attorney general, if necessary, to prevent so much from coming out. Dean replied that he was ready to inform the judge of the perils involved in releasing a lot of data about the atomic bomb, adding that if any problems arose during Koski's testimony, he (Dean) would go to New York personally to insure that no other secrets were compromised in the courtroom.²¹

Perhaps spurred by Hickenlooper's call, Dean toughened the commission's position, dispatching a stiff letter, drafted the day before by Denson and Rolander, to the Department of Justice. Dean reminded the department that the commission hoped "the scope of all testimony will be determined by that information transmitted by Greenglass to Russian agents." The commission saw "the inherent danger involved in the cross-examination of the prose-

²⁰ Commission Meeting 530, March 2, 1951; and Dean Diary, March 2, 7, 1951. The documents of the Atomic Energy Commission do not indicate what Saypol wanted to release in court.

²¹ Dean Diary, March 8, 1951.

cution's witnesses which may result in the disclosure of additional information; however, it is hoped that the direct examination of the prosecution's witnesses will be conducted with such care as to minimize the opportunities for defense counsel to obtain further disclosures." Again Dean reminded the Justice Department that the commission had not officially declassified the information which would be revealed in court, and he was anxious that no one draw the inference that any other information had been released. To demonstrate the commission's concern regarding speculation about the data released in New York, Dean enclosed a copy of the directive to commission employees. Going a step further now, Dean requested "that drawings, sketches, or documents introduced as evidence at the trial not be made available to the public insofar as possible." Finally, Dean asked for transcripts of the testimony of prosecution or defense witnesses who gave any evidence relating to atomic energy. Dean closed by reminding the Justice Department that the matter was of "extreme interest" to the joint committee as well as to the commission. Dean had taken his final stand.²²

But District Attorney Saypol refused to give the commission the questions he planned to ask Koski and Derry. When he learned that Koski's testimony had been postponed until Monday, March 12, Dean called Attorney General J. Howard McGrath and explained the commission's problems. At McGrath's suggestion, Dean and members of the commission staff met with the attorney general on the preceding Saturday. After numerous calls to New York, Dean was finally able to secure the questions Saypol planned to ask the technical experts Koski and Derry. The questions were revised in consultation with the other members of the commission staff over the weekend. On Monday Saypol handled Koski precisely as the commission wished, interrupting Greenglass's testimony to put him on the witness stand and sticking literally to the revised questions. To the commission's relief, defense counsel did not ask Koski any damaging questions on cross-examination. As the commission's general counsel later put it, Koski was brilliant and the commission staff in New York "seemed to 'light up' over night." With Koski off the stand, the rest of the trial went smoothly for the commission. As Dean later told Peyton Ford, "the whole complexion had changed" after the commission examined the questions for Koski and Derry. Later, Gordon Dean thanked Attorney General McGrath for his help.²³

For the Atomic Energy Commission the Rosenberg case was actually the Greenglass case. David Greenglass was a problem to the commission because his testimony was the touchstone from which atomic secrets used as evidence against the Rosenbergs would become public. The commission was not as

²² Dean to Ford, March 8, 1951.

²³ Dean Diary, March 9, 10, 12, 19, 1951; and Dean to McGrath, March 27, 1951. The Greenglass questions were not cleared through the commission. Dean Diary, March 12, 1951. Nizer calls Koski's interruption of Greenglass a well-conceived plan of the prosecution to establish Greenglass's technical credibility; see Nizer, *The Implosion Conspiracy*, 100. Perhaps it had that effect; the commission, however, certainly saw it as the safest way to get Koski on and off the witness stand. Judge Kaufman, who presided over the trial, was so impressed by Koski's testimony that, in a personal remark to William Denson, he (Kaufman) called Koski the best witness he had seen in his experiences as a judge. Rolander to Koski, March 23, 1951.

worried about what he would say—since it assumed that most, if not all, of his testimony had already been compromised—as it was concerned about what other secrets—especially levitation and further details about the “initiator”—might be compromised in the process of allowing him to testify. The two most likely sources of compromise were testimony by technical experts and public speculation by commission personnel. All of the security measures that the commission implemented were aimed at preventing slips from either of these sources. In essence, the commission confronted a dilemma typical of democratic societies: how to prosecute individuals believed to have stolen secrets, while protecting other closely related information and without infringing the right of the accused to confront incriminating evidence.

DID DAVID GREENGLASS TELL THE TRUTH? The records of the Atomic Energy Commission only tell us that the technological details about the atomic bomb in his courtroom testimony were fairly accurate. Precisely because he was telling the truth about the bomb, he created a problem for the commission. Greenglass had learned a major technological secret of the World War II atomic bomb project—implosion—and a new postwar technological improvement in atomic weapons—levitation. Implosion was a new concept developed during the war as a method of detonating an atomic bomb. It was the most efficient method of detonating any bomb developed during the war and the only method by which a bomb using plutonium could be detonated.²⁴ Greenglass’s information about lens molds and early implosion experiments helped show how it was achieved; hence, the need to classify and protect those details. The description of the atomic bomb gave an overall picture of how to use implosion to detonate a bomb, described some bomb parts, and gave a rough view of the shape of a plutonium bomb. Though not completely accurate, the description did reveal enough to be considered too sensitive to declassify officially in 1951. The information about levitation was of concern to the commission because it could lead to new improvements in weapons technology. (Even now levitation cannot be described or discussed here for the same reason that it was not used in court against the Rosenbergs—because of the potential for possible weapons design improvement.)

The Atomic Energy Commission, despite some anxious moments, was able to solve successfully the security problem posed by the Rosenberg/Greenglass case. David Greenglass and supporting experts Walter Koski and John Derry gave their testimony without any inadvertent revelations, and defense lawyers neither trapped them into further disclosures nor subpoenaed any physicists as defense witnesses. The levitation information was not used in court and remained protected. In fact, the Rosenberg lawyers insisted that the most sensitive part of the Greenglass testimony be impounded. Not one vital secret, other than what had already been compromised, came out in court. The commission could not have hoped for more.

²⁴ For an explanation of the novelty of implosion, its efficiency, and the necessity for using it to detonate plutonium, see Hewlett and Anderson, *The New World, 1939-1945*, 235, 245-49, 250-51.

Reviews of Books

GENERAL

FELIX GILBERT. *History: Choice and Commitment*. Cambridge: Harvard University Press. 1977. Pp. vi, 549. \$18.50.

Felix Gilbert is a master craftsman of history, and it is pleasant as well as useful to have this collection of products from his workshop. Of the nineteen articles most have been published before, a number are fundamental contributions to various areas of European history, and a few are classics. The topical arrangement does not suggest the development of Gilbert's work, but it does reflect his extraordinary range and depth. Moreover, the principal themes have been almost continuous over the five decades of his career: Italian political history, especially Renaissance Florence and Venice and Machiavellian studies, European diplomacy from that time down to the present, and many aspects of historical thought and scholarship. The book displays intellectual horizons and a depth of perspective to which few historians can aspire.

There have been many ways of trying to view the whole of "modern" European history. There is the 1453 school, and there is the 1455 school (if the invention of printing can be fixed so exactly). There is of course the 1492 school, the 1494 school, and the 1517 school; and there are groups, such as economic and social historians, who seem to lack a legitimate date of birth. Felix Gilbert belongs to the 1494 school. The reason is not only because this date marks a crucial point in diplomatic history and in the emergence of the European *Staatensystem*, but also because the Italian wars and their aftermath were seminal for modern political and historical consciousness. Gilbert's view, orthodox no doubt at Ranke's University of Berlin, where he studied with Friedrich Meinecke, was reinforced by an early interest in Italian art and in Guicciardini (even before Machiavelli) and, naturally, by his own *Italienische Reise*. In any case, after a thesis devoted to Meinecke's teacher Droysen (part of which is translated here for the first time), Gilbert

made a lifetime commitment to the "age of Machiavelli" and the problems it posed—and still poses.

Given his heritage and interests, a confrontation with Machiavelli was unavoidable for Gilbert, and out of this came a number of his most important contributions: his study of *The Prince* in terms of the political genre of the "mirror of princes," complementary articles on the *Discourses* and *Florentine Histories* as well as the later tradition of "Machiavellism," and above all his investigation of the intellectual circle of Bernardo Rucellai, which throws light on "the origin of modern political thought." A counterpart to the Florentine political tradition is that of Venice; and other well-known studies take up the constitution, the diplomacy, and the political "myth" of this city-republic. Other articles deal with the "new diplomacy" of the eighteenth century, Ciano and his ambassadors, and the history of modern historical scholarship and writing, including discussions of Angelo Poliziano, Lorenz von Stein (this again translated for the first time) and, finally and most appropriately, Gilbert himself, in a previously unpublished "Reflections on the History of the Professor of History."

Felix Gilbert's writing and teaching, though deeply involved with contemporary historical subjects and problems, represents a direct link with one of the grand traditions of professional history. As a student of Meinecke—and so a grand-student, we may say, of Ranke—he may be regarded as an alumnus, though to be sure a very critical alumnus, of the nineteenth-century German school of history. Without sharing the philosophical views of Ranke and his colleagues and followers, Gilbert acknowledges that "we are still their heirs insofar as we are professors of history" (p. 452). What is more, he has carried on this tradition not only by writing about Droysen, Meinecke, and that neglected master, Otto Hintze, but also by developing certain aspects of their approach to history and by pursuing some of their long-range concerns—the function of diplomacy in political history, concepts of "reason of state" and Machia-

vellism, administrative history, and the significance of nationalism.

Not, of course, that Gilbert accepts Ranke's view of "scientific" history (from which he is perhaps as remote as Ranke himself was from Guicciardini) but he does appreciate Ranke's role in professionalizing history and freeing it from idealistic presumptions. Nor does Gilbert follow the *ideengeschichtlich* approach of Meinecke. In fact he dissociates himself from his former teacher not only in his assessment of nationalism and *Staatsräson* but also on methodological grounds; for Meinecke, he points out, "did not pursue the study of intellectual trends to an analysis of the forces which they represented" (p. 87). It is precisely in the linking of intellectual history with social and political history, it seems to me, that Gilbert has made his most distinctive contributions. Like Ranke he makes use of archival materials and examines the political process of history; like Meinecke he also interprets the thoughts of participants in and observers of this process; and by combining the two, by investigating the interplay between history and consciousness, he has preserved the best of his (which is, in many ways, to say our) professional heritage. This is evident in Gilbert's masterpiece, *Machiavelli and Guicciardini*, as well as in these essays.

Professional history in this country was originally a European, and especially a German, import; and recurrently European influence has acted, often positively, to shape it. No such scholarly infusion has been more significant than that created by the generation trained in Germany in the 1920s, themselves under the influence of such teachers as Meinecke, Hintze, and especially (I think Felix Gilbert would say especially) Max Weber. And in the field of early modern history no scholar has been a more effective carrier of this influence than Gilbert (of his peers I think of Hajo Holborn and Hans Baron). He has taught us, and indeed still is teaching us, the value of large perspectives and large questions, yet always in the context of concrete and probing (often minutely probing) research. That is the calling of the professional historian: "*Le bon Dieu est dans le détail*," as Gilbert quotes Aby Warburg. So is life; and about the relationship between the professional *vita contemplativa* and our more active existence Gilbert's own conclusion is, characteristically, both positive and modest, "that the past is one way—and not the worst way—of acquiring the right and the criteria to judge the present."

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INA SPIEGEL-RÖSING and DEREK DE Solla PRICE,
editors. *Science, Technology and Society: A Cross-Disciplinary Perspective*. Beverly Hills, Calif.: Sage Publications. 1977. Pp. 607. \$29.95.

For more than three centuries science has exerted a cultural influence in Western civilization; only during and after World War II, however, has the intimate relationship between science and power—political, economic, and military—been clearly comprehended. In the past three decades governmental support of scientific research and development in both capitalist and socialist nations has increased enormously; rapid growth in the numbers of scientists, in the accumulation of scientific information, and of bureaucracies involved with science and science policy has occurred; and significant qualitative changes involving science education, research strategies, and the appearance of new scientific subdisciplines have taken place.

These developments have been paralleled by a substantial increase in the numbers of humanists and social scientists specializing in various aspects of the study of science, technology, and society (SSTS). Their activity has resulted in the development of teaching programs, the creation of research institutes, and the establishment of scholarly journals and both national and international organizations. Interdisciplinary integration of SSTS is, however, still an acknowledged deficiency and a coveted goal. Directed primarily toward scholars working in the various disciplines comprising SSTS, this volume, composed of "state of the field" essays, is intended to contribute to an intellectual synthesis.

The book is divided into three sections. The first, which describes SSTS and current trends in the relation between science and society, includes three articles: "The Study of Science, Technology and Society: Recent Trends and Future Challenges" by I. Spiegel-Rösing (Universität Ulm), "Science Policy Studies and the Development of Science Policy" by J. O. Salomon (OECD and CNAM, Paris), and "Criticisms of Science" by J. Ravetz (University of Leeds). Articles recapitulating, analyzing, and summarizing the results of research in each of six scholarly disciplines involved in the social study of science comprise the second section. They are: "Sociology of the Scientific Research Community" by M. J. Mulkay (University of York), "Changing Perspectives in the Social History of Science" by R. MacLeod (University of Sussex), "Conditions of Technological Development" by E. Layton (University of Minnesota), "Economics of Research and Development" by C. Freeman (University of Sussex), "Psychology of Science" by R. Fisch (University of Konstanz), and "Models for the Development of Science" by G. Böhme (Max-Planck-Institut,

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Starnberg). The third section is composed of six articles addressing issues or treating areas in which science policy has become of paramount importance. They are: "Scientists, Technologists and Political Power" by S. Lakoff (University of California, San Diego), "Technology and Public Policy" by D. Nelkin (Cornell University), "Science, Technology and Military Policy" by H. Sapolsky (MIT), "Science, Technology and Foreign Policy" by B. Schroeder-Gudehus (University of Montreal), "Science, Technology and the International System" by E. B. Skolnikoff (MIT), and "Science Policy and Developing Countries" by Z. Sardar and D. G. Rosser-Owen (Muslim Institute, London).

The organization of the volume discloses one major dilemma confronting the integration of SSTS. SSTS comprises two scholarly areas: disciplinary studies, the goal of which is to determine by diverse approaches how the scientific enterprise functions; and policy studies, which are concerned with how science can be controlled and directed. As Spiegel-Rösing puts it (p. 17), the first group is involved with cognitive problems and the second with operational problems. If only the synthesis of these two perspectives stood in the way of integrating SSTS, then some degree of success might be achieved. There is, however, a more basic dichotomy. The first group is concerned primarily with *science* and the second with *technology*, i.e., the former with the conditions under which new scientific knowledge is obtained, and the latter with the development, application, or use of technologies in which scientific knowledge may be involved in varying degrees or not at all. Layton's article describes some studies which have sought to determine under what circumstances scientific knowledge finds technological application; he concludes, however, (p. 217) that the relationship between science and technology is "still mystical."

The conflation of science and technology gives credibility to a recurring theme in the volume, which is that morality and social utility should be the primary purposes of science. In fact, the assertion voiced by many scientists that pure science is morally neutral provokes impatience or even annoyance in several essays: e.g. Salomon (p. 65), Ravetz (p. 85), Fisch (p. 298), and Böhme (p. 320). There seems to be a refusal to accept the ambiguity inherent in science, which is, as is the university, a social institution from one perspective, and from another an autonomous enterprise.

With few exceptions, the essays are comprehensive in scope, concise, and clearly written. Cross-referencing is employed frequently to avoid repetition, and the bibliographical citations given by the authors must number more than two thousand.

This volume will be useful not only to historians of science and technology, but also to social and diplomatic historians.

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JOHN FARLEY. *The Spontaneous Generation Controversy from Descartes to Oparin*. Baltimore: Johns Hopkins University Press. 1977. Pp. x. 225. \$13.50.

This book will surely prove to be the standard work on its topic for a long time to come. Most of us, I suppose, even those of us who are not historians of science, "know" all about the spontaneous generation story. For nearly two millennia, starting with Aristotle, people believed that organisms were being created continuously from inorganic matter. Then along came Francesco Redi in the seventeenth century, Lazzaro Spallanzani in the eighteenth century, and, as a grand climax, Louis Pasteur in the nineteenth century. From insects, through infusorians, to bacteria, the old beliefs were destroyed by means of experiments, which are still taken today as textbook paradigms of scientific methodology and objectivity (at least, they were taken as paradigms in my text-book!). Somehow the story is just a little too neat to be true, and as John Farley shows with devastating erudition—acquaintance with texts in several languages and a profound knowledge of technical science, sadly all-too-often lacking in historians of science—the truth is indeed far from the popular picture.

First, rather than a smooth upward progression from falsity to truth we have a series of cycles, as belief in spontaneous generation waxed and waned in the minds of scientists. For instance, at the beginning of the nineteenth century, *after* Redi and Spallanzani, belief in spontaneous generation flourished as never before: it was accepted by nearly all the major natural historians. Second, rather than a model of pure objectivity we have belief in spontaneous generation accepted or rejected for all sorts of extrascientific reasons like philosophy and religion. Early nineteenth-century thought swung toward a spontaneous generation belief primarily because of *Naturphilosophie*, which emphasized the unity of everything and denied the distinction between life and nonlife. On the other hand, Cuvier rejected spontaneous generation because he believed that life and only life showed an integrated functional teleology: to him spontaneous generation was not just empirically false, it was a contradiction in terms.

Third in Farley's debunking of popular history, he shows that rather than succumbing to an ultimate *coup de grâce* by Pasteur, the belief in sponta-

neous generation lingered right into this century. It was the Marxist scientist A. I. Oparin who finally destroyed the belief, as he argued from a dialectical materialist position that life's origin requires a gradual process of chemical evolution and cannot be just a one-step affair. This means, therefore, that even now the spontaneous generation issue rests less on matters of brute fact and more on philosophical commitment (although one guesses not a commitment that most Western scientists would be very happy to admit). "Contrary to popular belief . . . the present state of the controversy has not resulted from disproof by infallible experimental evidence. The issue is one which has been abandoned many times before, only to reappear at a later date under a different guise. Whether the final chapter in the history of the spontaneous generation controversy has now been written it is impossible to say" (p. 187).

I urge you to read this fine book and to encourage every biologist you know to do the same.

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OWSEI TEMKIN. *The Double Face of Janus and Other Essays in the History of Medicine*. Baltimore: Johns Hopkins University Press. 1977. Pp. x, 343. \$22.50.

Owsei Temkin has at last published a volume of essays from his forty-five years of scholarly research and teaching in Germany and the United States. The choice of papers is excellent. Their themes, style, and quality are remarkably consistent considering that this period has been one of transition—from traditional to hospital medical practice, from scientific breakthroughs to the industrialization of medical science, from the "few good years of the Weimar Republic," with its faith in "men against death" to current disillusionment with the social and individual meaning of medicine.

In the introductory essay entitled "The Double Face of Janus," Temkin tells his own history. Born to a family of Russian Jews who emigrated to Germany to escape pogroms, Temkin found his first home in the University of Leipzig, where he developed his skill in both medicine and philosophy at Sudhoff's Institute for the History of Medicine under the man who remained his chief until 1948, Henry E. Sigerist.

During what Temkin describes as the romantic years of Weimar, Sigerist and Temkin developed their talents for positivistic historical research. Sigerist, the first professional director of the Institute for the History of Medicine at Johns Hopkins University, brought Temkin to the United States in 1932, just before Hitler seized power. Sigerist be-

lieved in the medical usefulness as well as the intrinsic scholarly merit of Temkin's knowledge of ancient and scholastic traditions, and had great hopes for his forthcoming study of the history of epilepsy. "The Double Face of Janus" refers to Sigerist's challenge to develop this usable past for the medical profession. In these thirty-six essays (previously published, mainly in the Institute's *Bulletin for the History of Medicine*), which employ almost every tool of intellectual history from textual analysis to "influence studies," Temkin does considerably more.

Temkin consciously turned to philosophical history to clarify the central concepts of medicine: health, disease, and man. Two of his best essays, "An Historical Analysis of the Concept of Infection" (1953), and "The Scientific Approach to Disease: Specific Entity and Individual Sickness" (1963), reveal tensions in these concepts basic to medical thought and significant for its practice: "There is no meaningless universe in medicine." In all his papers, which are discursive rather than definitive, Temkin relates this universe to his humanistic philosophy. Key words in this task are moral: meaning, culture, responsibility, morality (rather than ethics), interrelationships, and humanism. The disciplinary consequence of this choice is that Temkin has remained skeptical of the social sciences, in regard both to Sigerist's prewar enthusiasm for socialized medicine and to the current hopes for social history.

In "The Meaning of Medicine in Historical Perspective" (1965), Temkin justified his bias in terms of the complexity and elusiveness of medicine in culture. His best essays on particular topics make the point more effectively: the mainstream of modern medicine is understandable only in terms of the traditions that have guided it and, at times, raised emotional and philosophical obstacles in its path. Temkin is well known within the history of medicine for his masterly unveiling of the subtleties and pervasive influence of the Galenic and Hippocratic traditions in medical culture, represented here by essays on "A Galenic Model for [William Harvey's] Quantitative Physiological Reasoning" (1961), and "Byzantine Medicine: Tradition and Empiricism" (1962). One may object to the first that there is no evidence that Harvey was directly influenced by Galen's model even though he was, as Jerome Bylebyl has shown, firmly caught in the Galenic and Aristotelian medical debates of his time. The same objection can be made to Temkin's interpretation of nineteenth-century biology in essays seventeen and twenty-four to twenty-eight. While admiring his brilliant recognition of the emotional climate produced by the *idéologues* and *Naturphilosophs*, one feels acutely the gap between these philosophies and the actual

concerns of François Magendie, on the one hand, in nutrition research, and of the German Darwinians, on the other hand, in phylogeny.

Temkin confesses his own attraction to "what is obscure and slightly exotic," but he refutes any potential judgment by sociologist or biologist of its irrelevance. He does not, to this reviewer, succeed because he has fulfilled Sigerist's demand to give the history of medicine its Janus face of current use within medicine. Temkin has rather repeated the virtuoso performance he recalls from Sigerist in 1925: "It was he who first made me see a mental picture of medicine, i.e. it was he who changed the past into history." Temkin, the master of the traditional archive, is also the artist who captures our historical imagination and converts potential gaps (such as that between the philosophy and research of Magendie) into well-defined challenges for future scholarship. Other examples of this artistry useful to all historians are essays in which Temkin makes explicit his historical framework and periodization, "Greek Medicine as Science and Craft" (1953), and "On the History of 'Morality and Syphilis'" (1927).

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SALLY SMITH HUGHES. *The Virus: A History of the Concept*. New York: Science History Publications. 1977. Pp. xix, 140. \$6.95.

As the "Concept" of the title suggests, this is an internalist history of an idea. Well organized, and written in a lucid if not exciting style, *The Virus* nicely illustrates the benefits of such an approach—a well-researched genealogy of our present notion of "virus"; a delineation of the problems, both conceptual and technical, which had to be overcome along the way; and, as a result, a source for understanding better the meaning and implications of the present definition of "virus." As such, the work will be inherently interesting to the scientist and an important resource to the historian.

The Virus also demonstrates the shortcomings of a history which is strongly oriented to the internal logic of the field. Using the present notion of virus as a guide, it tells us how our ancestors were heading for where we are now, as well as how they were departing from where they were then. Thus, not all of Sally Smith Hughes' criteria for what is central to her story select what would have been relevant to those engaged in such studies at the time. Moreover, as often happens with this approach, the author does not always resist measuring the success or failure of a particular view or experimental result against modern knowledge.

This leads her, at times, to indulge in the issues of "right" answers and priority disputes. A more harmful consequence of this restricting framework is that, as the story approaches the present, it gradually slips into a mere cataloguing of events, a setting out of milestones along a thin ribbon of prose.

These complaints need to be balanced with the admission that the historian of science is often caught between two readerships and cannot hope to please them both. The more presentist the approach, the more likely the work will be read by scientists and others interested primarily in that scientific (as opposed to historical) subject. At the same time, such work will be seen by the historian as a bit artificial and unsatisfying. On these grounds, maybe Hughes, herself a microscopist, is entitled to ignore the above reservations as arising from a misunderstanding of her intentions. Moreover, she would be quite right in suspecting that the historian, trying to wrestle with any subject in this period (the late nineteenth and twentieth centuries) which is related to her topic, will straightaway head for her book, and will find it very useful.

DONALD G. BATES
McGill University

TORSTEN ERIKSSON. *The Reformers: An Historical Survey of Pioneer Experiments in the Treatment of Criminals*. Translated by CATHERINE DJURKLOU. New York: Elsevier. 1976. Pp. 310. \$17.50.

"Writing this book," Torsten Eriksson, a former director of the National Swedish Correctional Administration, notes in his preface, "has been rather like solving a jigsaw puzzle. Countless pieces of information have been picked up here and there and gradually fitted together to form a whole." His claim is only half accurate. Countless miscellaneous pieces of information are to be found in this book but they hardly add up to a coherent examination of criminal justice reform or experiments in the treatment of criminals. What we have instead is an assortment of brief sketches of European and American attempts at rehabilitation, a catalogue of the most unforgettable reformers and their prisons through the ages.

That this book has been translated from Swedish into English is probably testimony to the paucity of solid historical research in this field. The best that can be said for it is that perhaps after reading it some students will be prompted to undertake further investigations. Not that they will find here a framing of the right questions to ask, let alone suggestions as to how to begin to answer them. Indeed, they will not even have the advantage of an up-to-date bibliography or references to

current work. But they will obtain some sense of the variety of attempts in Europe and the United States to reform the deviant. Eriksson does move in rapid-fire fashion from the early institutions for the vagrant in Amsterdam to the travels and work of John Howard, the Norfolk Island experiment of Alexander Maconochie, the Irish Progressive system of Walter Frederick Crofton, the efforts of Zebulon Brockway at Elmira, and on to the current "therapeutic communities" in Scandinavian countries. The very diversity and persistence of the enterprises may spark some interest.

And the students who happen on this volume may be encouraged to formulate some of the critical questions, even if the author does not. They may wonder at the book's dedication: "To those who tried, even if they failed." Why was it that good intentions did not often manage to produce desirable results? Why is it that time and again the first quotation from a contemporary observer that Eriksson provides is about the humanitarian goals of the proposed prison program and the second is about its subsequent corruption and brutality? They may also ask why it is that reform currents change. Why in the late nineteenth and early twentieth century did reformers focus more on release procedures than on institutional routines? And they may wish to explore the social and cultural assumptions that underlay successive reform programs. As Eriksson writes it, the history of criminal justice reform is the story of sporadic manifestations of humanitarian impulses, or else, making progress when "the time was ripe for innovation" (p. 27). But students may want to ask why benevolent programs take different forms in different periods, and just what is it that makes a time "ripe" for innovation.

In the meanwhile, until more of this work gets done, we must hope that bad books are not like bad currency, that they will not drive out competent researchers from so important a field.

DAVID J. ROTHMAN
Columbia University

FERENC TÖKEI. *Zur marxistischen Geschichtstheorie*. Volume 1, *Zur Theorie der Gesellschaftsformen*; volume 2, *Antike und Feudalismus*; volume 3, *Zur Dialektik des Sozialismus*. Translated by ZOLTÁN PAULINYI. Budapest: Akadémiai Kiadó. 1977. Pp. 149; 196; 127. \$9.50; \$12.50; \$8.00.

In his *Adventures of the Dialectic* Maurice Merleau-Ponty remarks that, while the dialectic appears in the later works of Marx and Engels, "it is no longer a paradoxical mode of thought, the discovery of an entangling relationship between the dia-

lectic and his object, the surprise of a spirit which finds itself outdistanced by things and anticipated in them. It is the simple verification of certain descriptive features of history, even of nature." This judgment applies with a vengeance to Ferenc Tökei's three volumes, whose shared object is the Marxian theory of history itself. Clearly written and attractive (although lacking index and bibliography), they present in the author's words "orthodox" elaborations on the classic texts and themes of Marx, Engels, and periodically Lenin. Tökei indicates that, since his focus is "philosophical-methodological," his studies have an abstract character. This is not the problem. Reduction of forms of society and consciousness to forms of production and property relations is.

The *Theory of Social Forms* never emerges from the rigid conceptual chrysalis of economic "base" and ideological "superstructure"; nor does it critically examine many of the long-standing problems of the nomological view of historical materialism. In the *Antiquity and Feudalism* volume, the richest of the three, Tökei does not pursue the issue raised sixty years ago by Georg Lukács: that Marxism's very historicity prevents its direct application to pre- (and post-) capitalist societies which, by definition, are not dominated by economic relations. "Society, Art, Religion" is relegated to a brief, final chapter. The recent *Passages from Antiquity to Feudalism* by the British Marxist historian, Perry Anderson, constitutes an implicit criticism of Tökei's monocausal, linear, necessitarian assumptions. Since he came to the defense of the concept of "the asiatic mode of production" in an earlier volume (1969), Tökei does not take up this vexing question in the present studies. From *Dialectic of Socialism* readers should not expect discussion of the problems raised by contemporary socialist societies, as this is not the author's aim, although in an appendix he defends Lenin's view of "cultural revolution" against those "East and West" who would employ it as a critical standard of judgment upon existing socialist regimes. I found this, and Tökei's uncritical exegesis of the "two-stage" theory of proletarian revolution, often used to justify repression, painful.

Since the stated aim is to articulate the orthodox case for the Marxian theory of history, Tökei's volumes are at first glance successful. Yet, the question may fairly be asked, what is the orthodox case? If it amounts to "annexing Hegel's logic to the economy" and displaying the purported *Ratio* of matter and forms of production, then Tökei scores high. If, however, it entails "recreating the dialectic by entrusting it to the very movement of its content" (Merleau-Ponty), then for all his learning and experience, Tökei fails. In the end, his studies do not match the standards set by

Helmut Fleischer's little book, *Marxism and History* (1969; Engl. ed., 1973).

PAUL BREINES
Boston College

HAL DRAPER. *Karl Marx's Theory of Revolution*. Part 1, *State and Bureaucracy*. In two volumes. New York: Monthly Review Press. 1977. Pp. 383; 385-728. \$28.50 the set.

In his foreword, Hal Draper promises "a full and definitive treatment of Marx's political theory, policies, and practice" (p. 11). It is an important subject, which, as he correctly says, published studies have left unexamined, although Shlomo Avineri's *Social and Political Thought of Karl Marx* (1968) most illuminatingly comprehends the first two elements. But then Draper's title differs from his intent, and even if that could be explained, the subtitle adds two other subjects which also require a justification they never get. Moreover, the author gives outlines (pp. 27-28) of future volumes indistinctly related to these first two. Draper has launched himself in all directions.

The book does not have a subject. In this order, with frequent backward glances as it moves on, it discusses Marx's youth, his early writings, the Revolution of 1848 and Louis Napoleon, the Paris Commune, Oriental despotism (as Marx saw it in his *Grundrisse*), and tsarism. It reads like a series of indoctrination talks aimed at Marxists who have some knowledge of Marxian theory but require further elucidation. Draper seems to feel that he must show how apparently incongruent later events actually fit into the system created by Marx. The book's structure, to the extent that it has one, cannot contain all this, and its author has been forced to add six irrelevant or repetitious appendixes totaling seventy-four pages. Within the text he feels obliged to change his themes continually—now defending Marx against the accusation of being simply a passive determinist (like many German Social Democrats) or a reality-shattering voluntarist (like Lenin); now fending off charges that Marx was anti-Semitic or anti-Russian, ineradicably Hegelian or transcending all philosophy, or self-contradictory. Since all these statements are true enough and, taken with what Marx accomplished, can be seen as further demonstrations of his genius, the author can only lose one defensive encounter after another. In the process Draper loses his subject.

The book has one extraordinary characteristic, its time context. Its perspective is that of the 1930s: Draper articulates a discouraged attitude typical of the Great Depression, while his general references, examples, similes, metaphors, and even ex-

pressions of humor relate to that period. His development—and nondevelopment—of arguments reinforces the sense of anachronism.

While defining himself explicitly as a Marxist, Draper is vague about just where he stands on his Marxian ground. He defines his position negatively for the most part. He rejects much of "what goes by the name of Marxism nowadays [as having] little to do with Marx's views. . . ." (p. 17). He writes that Lenin's *State and Revolution* was his methodological guide in research and writing, but has little to say about Lenin as leader and theorist. Draper avoids expressing any attitude about Stalin. The bibliography lists a modest number of secondary works, but the text is written as if Draper were unaware of their existence. It is almost pointless to note that the bibliography does not include Lukács and many other distinguished Marxian intellectuals. Situating himself in a period in which no man lives, Draper has virtually sealed himself off from the world, capitalist or Marxian, in a personal Marxian time capsule.

Draper's book has a certain value as an ideological artifact in a museum of Marxian antiquities. To a non-Marxist, it can suggest the type of thinking he will sometimes encounter in the Marxian world when reality—or a scholar like Lukács—does not overwhelm it. Those who would like to understand Marx's ideas about revolution, besides referring to Avineri, should go to Marx himself. Convenient English-language selections of his revolutionary and political writings (the adjectives are synonymous: Marx's politics was only the politics of revolution) are Saul K. Padover, ed., *On Revolution*, and David Fernbach, ed., *Karl Marx . . . Political Writings* (3 vols.).

This is only the beginning. Marx was a leader who shaped his words and even his thoughts to suit his political purposes. To understand his "theory of revolution," one must study what he did. Draper promises to do so when he puts Marx's "practice" in his statement of intent, but he never gets to it, and his outlines of future works barely suggest it. The task remains to be undertaken.

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WILLIAM WISELEY. *A Tool of Power: The Political History of Money*. New York: John Wiley and Sons. 1977. Pp. xi, 401. \$16.95.

Like war to the generals, it appears that international money is too important a subject to be left to the economists. Political scientists, historians, and even sociologists begin to find in the subject a rich field for applying their disciplines.

William Wiseley's contribution to this move-

ment is ineptly titled. Two-thirds of the book are devoted to a detailed account of international monetary negotiations from Bretton Woods to Jamaica in January 1976. These are preceded by a potted history of copper, silver, gold, and bimetallic standards and banking. Money is not shown to be a tool of power: Lenin and Hitler achieved power without it, and the United States in the interwar period avoided power with it. To a considerable extent Wiseley is preoccupied with the role of gold in the international monetary system. I find dubious generalizations like the author's assertion that the French propensity to hoard goes back to the fourth century (p. 19), or that French governments are bullionist because French people trust their governments less than the British do theirs (p. 36).

The point of view is strongly nationalist, anti-European, and very anti-French. Wiseley believes that the United States has always been milked and bilked by its allies who extort ransom. He believes that it should have withdrawn its troops from Europe long ago, and quit the International Monetary Fund. The French are variously described as pedantic, frivolous, arrogant, stubborn, greedy, rapacious, selfish, making absurd demands, showing astonishing gall, and importunate. Ten United States presidents in unbroken succession since 1914 lost angry battles with the French about money (p. 309). When the French show remarkable recovery, it is not owing to skillful management, but to natural advantages (p. 194). Wiseley admires Adenauer, Jacobsson, Arthur Burns, Henry Reuss, George Shultz, and John Connally, detests all Frenchmen (especially Giscard d'Estaing, Michel Jobert, and Pierre-Paul Schweitzer), William Yandell Elliott, Henry Kissinger, and Anthony Eden.

The sources are newspapers and other authors, cited for the most part in their entirety or in large chunks of pages, rather than in pinpointed references. An economist-reviewer is put off by solecisms such as "printing presses of the mint" (p. 114), Morgan Brothers (pp. 121, 153), Lord Walter Cunliffe (pp. 84, 96), the suggestion that the Bank for International Settlement was a prototype for German clearing-houses (p. 137), etc. Many technical issues are referred to without full explanation: the need for symmetry in the system, the question of the level of interest on Special Drawing Rights, the difference between a reserve asset and a credit. There is contradiction: when the French held sterling, Britain was dependent; the holding of dollars made Europe dependent on the United States. Kissinger suffered from lack of knowledge of economics; Connally's ignorance was an advantage. Overvaluation helped Russia, hurt the United States.

But for all its subjectivity and weaknesses, the book is helpful as an account of the monetary storms of the Bretton Woods period. The research was done in Europe and some of the details and political color are new and interesting. It is too bad that the index is so uneven, excellent on names, and appallingly weak on subjects.

CHARLES P. KINDLEBERGER

Massachusetts Institute of Technology

ALAN S. MILWARD. *War, Economy and Society, 1939-1945*. (History of the World Economy in the Twentieth Century, number 5). Berkeley and Los Angeles: University of California Press. 1977. Pp. xiv, 395. \$12.95.

Like other authors in this excellent series, Alan S. Milward has attempted more than a survey of the main economic events in his period. He has given us an admirable state-of-the-arts report on what we know about how agriculture, population, technology, labor, industrial production, and public finance were affected by the war. He also sets out some highly challenging findings concerning the rationale and effectiveness of economic strategy as applied by the main powers. And he has tentatively advanced some large concepts about the nature of advanced economies as revealed by the manner in which they strove to cope with the war. His approach is broadly comparative: he gives us an account not only of the relative economic performance of individual European powers, but also of the Japanese and American war economies, plus a few observations on the situation in many smaller countries from Australia to Yugoslavia. The book is a mine of information and arresting concepts. It will be a rare teacher—even if he has little interest in economic history—who cannot improve his lectures on the war after working through this book.

It also will be a rare historian with some stake in World War II who will agree with all of Milward's conclusions. He asserts that war had "positive" economic effects (in a developmental as well as an income sense); that both strategies and the actual functioning of affected institutions were "normal" (expressed also as "undistorted"); that the "slack" (unemployed capacity) in the German economy before 1942 explains why Germany failed before that date to change from "mere exploitation" (looting) to sophisticated integration of occupied territories; and that the amazing resiliency and flexibility of modern advanced economies in general explains more about Nazi economic accomplishments than does the totalitarian nature of the Nazi system in particular. There is also a nasty "counterfactual" argument hidden

behind some of his findings: that in the absence of war the Western economies would have been no richer and in no better shape after 1945 than they were before 1939—though he states, in the case of technology, that “there is no convincing evidence that the overall speed of technological advance was greater in wartime” (p. 180).

Possibly the most engaging feature of this book is Milward's convincing demonstration that differences in economic concepts as well as economic resources helped shape different general strategy as well as different economic strategy. On details of his analysis Milward is less convincing. He is not careful about defining key terms. He slips disconcertingly from statements about episodes in a single country to generalizations about the Western economic system. He makes little attempt to bring in concepts of social cost when discussing “positive” aspects of warfare. But no one will quarrel with his complaint that most accounts of economic aspects of the war—usually written by political or military historians—incorporate serious misconceptions and grotesque oversimplifications. He has done us a service not only by challenging the clichés but also by giving us information and inspiration to improve upon them.

MARTIN WOLFE
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ANCIENT

JOACHIM HOPP. *Untersuchungen zur Geschichte der letzten Attaliden*. (Vestigia, Beiträge zur alten Geschichte, number 25.) Munich: C. H. Beck'sche Verlag. 1977. Pp. xii, 167.

Approximately three hundred inscriptions and five hundred passages in ancient literary sources are the fundamental basis for this meticulous examination of the fragmentary evidence relating to the Hellenistic kingdom of Pergamum during its zenith of power in the second century B.C. An amplified dissertation, it bears the marks of expert guidance by Hermann Bengtson. Although an alphabetical bibliography of secondary authors is lacking, profuse notes cite all relevant publications of the past century dealing with eastern Mediterranean diplomacy of this period, including very recent journal articles. One of the chief merits of the book is a complete index of all inscriptions and ancient authors used.

Joachim Hopp aims at a more systematic and detailed analysis than any previous reconstruction of information about the last two kings of Pergamum, Attalus II and III. He achieves this goal admirably and his work deserves a place in univer-

sity libraries and in future bibliographies. Only specialists in the Hellenistic era will find this study engrossing, however, for its precise attention to political-diplomatic minutiae allows little notice of patronage of culture by the Attalids or of economic and social questions.

In hop-sotch organization that has a real logic of its own, the author begins with an analysis of all evidence fixing the death of Eumenes II in 159 B.C. and with encyclopedic summaries of data on the parentage of Attalus III, and about his mother, grandmother, and two uncles, before going back to a series of reviews covering Attalid relations in 188–159 with various Hellenistic monarchies, Greek leagues, and Rome. Then, more chronologically, Hopp correlates the tenuous evidence on the wars and diplomacy of Attalus II, the brief reign of Attalus III, the will turning over the kingdom to Rome, and finally, still in detail, the revolt of Aristonicus, claimant to the throne in 133–129 B.C.

Hopp has little new evidence and does not venture any major interpretations which seem fresh, but he does contribute to resolving numerous difficulties—no mean accomplishment considering the limited evidence. He brings into focus several minor personalities who appear in the inscriptions.

At some points Hopp's conclusions are disputable. He credits the Romans with a cool calculation and consistency which seem unlikely. On page 56 he denies that Eumenes II followed an “*unabhängige Politik*” relative to Rome, while on page 68 he describes Eumenes' general policy as an “*unabhängigeren Kurs*.” Yet his publication is a solid contribution to further discussion of such questions.

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ALAN WARDMAN. *Rome's Debt to Greece*. New York: St. Martin's Press. 1976. Pp. xix, 201. \$14.95.

Some years ago, this writer made a note that Roman ambivalence toward Greek learning might make a good thesis or dissertation topic. Scholars have always seen the ambivalence, of course, but a study seemed worthwhile. Alan Wardman has now done it and well, though he did not make use of the quotation from Cicero, attributed to his father, that caught my notice (*De Oratore*, II:36,265): “The men of our time are like Syrian slaves; the more Greek they know, the worse they are.” This from a Roman who admired much that was Greek.

From the title one would expect that this book would show how the Romans used the enormous and diverse Greek cultural legacy. Instead, it is

primarily, as the first sentence of the preface says, "a brief outline of Roman opinions about the Greeks and Greek thought"—quite a different thing. Chapters deal with various topics; most important are those on Greek history and historians, rhetoric, and philosophy.

Some of the early material is a bit frivolous: is it really significant that Romans preferred Italy to Greece and said so? If a Roman general disciplined a Greek for disobedience, did it really imply anything ethnic? What if it had been a Roman offender? And there are a couple of early errors: Sallust thought that wealth first began to take its toll on the Roman character after the destruction of Carthage, rather than in Sulla's time (p. xiii); and it was not only the Epicureans who were expelled from Rome in 161 B.C. (p. xv).

The book quickly gets better, however; the best chapters are in the middle and latter sections. Wardman knows the literature well, and he achieves occasional illuminating psychological insights into the Romans whose work he examines.

The themes stand out: the Roman attitudes were colored by their belief in deeds, not words, experience, not theory, as well as by a conviction that their own heroes were as great as Socrates or Alexander—just not so extravagantly praised. To assuage their feelings of inferiority, Romans deprecated the Greeks at the same time that they admired them. When attempting any analysis of Greek thought Romans tried, with little success, to add something original and distinctively Roman, or they asserted Roman superiority. Sometimes they did not even understand the Greeks because of their mind-set. Such perceptions are not new, of course, but on the whole Wardman draws them carefully and interestingly.

Except for minor flaws, then, and a title that is somewhat misleading, this is a well-done, civilized book, with clearly set out themes that even undergraduates will easily be able to grasp, whether or not they understand the allusions and the subtler points.

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KEITH BRANIGAN. *The Roman Villa in South-West England*. Bradford-on-Avon: Moonraker Press; distributed by Humanities Press, Atlantic Highlands, N.J. 1976. Pp. 127. \$8.50.

In the brief span of its prime, the varied area from Hampshire to Land's End, particularly the countryside around Bath, could boast the greatest concentration of Roman-style rural residences in all Britain. Owners of these opulent homes undoubtedly

included some early Roman colonists and indigenous Romanized Britons, but most were probably wealthy émigrés from northern Gaul who came to Britain about 270–90 A.D. and settled on lands formerly under direct imperial control. Not all the two hundred seventy southwestern villas, about one-third of the identifiable national total, were elaborate structures, but more apsidal dining areas, polygonal bath suites, colonnaded porticoes, and floor mosaics have been excavated there than elsewhere in the whole province.

The mosaics are particularly interesting. About forty percent of the region's villas housed such patterned or scenic flooring, while only ten percent of the rest of the island's villas were so graced. Thriving stylistic schools at Dorchester and Cirencester performed most of the work. Some of the figures depicted—Europa, Orpheus, and Bellerophon—seemingly had special Christian connotations. These, and more explicit associations, as well as evidence excavated since this book was written, indicate that early Christianity, long considered an urban phenomenon, had already made considerable headway in the countryside. Local cemeteries suggest that peasants and laborers, as well as wealthy planters, had adopted the faith in large numbers.

Some villas were country houses; others were positioned to exploit tin, shale, building stone, and coal resources; but most were on self-sufficient farms growing wheat as the principal cash crop. Two hundred fifty acres cultivated in long rectangular fields would normally yield a surplus of some eight hundred bushels. Cattle were also raised to fill military orders for leather. Many mansions suffered in the devastating barbarian raids of 367, but most recovered. Nevertheless, the boom was over and living standards declined. Fires made on mosaic floors and untended roofs and baths suggest that the original owners abandoned these homes for the greater security of towns. Agricultural yields were maintained, however, because urban communities continued to require supplies throughout the fourth and fifth centuries. In fact, as economic units these estates showed remarkable longevity and some survived to become medieval parishes and manors.

Keith Branigan, now professor of archeology at Sheffield University, perceptively analyzes all these changes. He might have more frequently compared his observations with other regional characteristics or with the province as a whole, but he is undoubtedly correct that intense examination of several regions must precede new generalizations. Then too, a countryside perspective can be found in the writings of A. L. F. Rivet, to whom this book is dedicated. Inevitably archeology offers precious little about the personal lives of villa in-

habitants, but this investigation substantially enlarges our understanding of the immediate surroundings in which such individuals lived. This is a splendid book, and since Branigan is a prolific writer with a wide range of archeological interests, readers can look forward to more such stimulating studies.

EDWARD J. KEALEY
College of the Holy Cross

MEDIEVAL

STEVEN RUNCIMAN. *The Byzantine Theocracy*. New York: Cambridge University Press. 1977. Pp. viii. 197. \$9.95.

For his Weil Lectures in Cincinnati in 1973 Steven Runciman sketched the history of the Byzantine imperial office in terms of its Christian ideological theory and practice. As he says in his opening sentence: "The aim of this short study is to give an account of an Empire whose constitution, to use too legal a word, was based on a clear religious conviction: that it was the earthly copy of the Kingdom of Heaven." And, he reminds us in his concluding sentence: "No other constitution in all the history of the Christian era has endured for so long."

Runciman traces the shaping of a rationale which was formulated early, out of pre- and non-Christian sources, which had to be tested against Roman legal and cultural traditions, which was refined and qualified over the centuries, but which was not substantively altered down to the empire's end, notwithstanding the emergence of a small but potent tradition of opposition. The original formulation was what Runciman calls "the Eusebian constitution," which—based on Hellenistic royal theory—saw the sovereign as God's viceroy on earth, and his realm as the earthly imitation of the heavenly kingdom. The working out of this concept's implications required centuries, during which the emperor proved unable to enforce religious unity, and was shown to be not free to tamper with religious doctrine or to break the law by his personal conduct. Nevertheless, his position remained unique, and not even the patriarch could match or rival it as a religious counterpart. The only lasting opposition was monastic, crystallized by the early ninth century during the iconoclastic controversy; and, whatever its shifts and drifts, it remained ready to challenge the emperor as suitable issues arose. Yet, not even the monks could totally shake public acceptance of the emperor's Eusebian stature down to the end.

The book's title suggests rather more than one finds. The application of the word "theocracy" to

the Byzantine sovereignty deserves some discussion, but the author never provides it, or even uses the word in his text. His explication of the Byzantine theories of Christian kingship in terms of historical development and constitutional exegesis has not been done before in this way, to be sure. Nevertheless, the theories and ideas offered are essentially his synthesis of the work of others, as he frankly acknowledges through his citations. Occasional flashes of insight or illuminating turns of phrase, however, together with the book's convenient scope, do make this a valuable and stimulating survey. Students and the general reader will particularly find it helpful.

For a work on this scale, it is surprising how few inaccuracies have crept in, and it is less misstatements than ambiguities that caught my notice along the way. Thus, I am not sure what "authority . . . which the Bishop of Rome enjoyed over the whole Western Church" is meant when the author speaks of Emperor Maurice's supposed plan to give parallel "authority" over the Eastern patriarchs to the Patriarch of Constantinople (p. 52). Emperor Leo V was not exactly "brutally murdered in church by his aide-de-camp, Michael the Amorion" (p. 87), but by supporters of Michael, who himself was languishing in prison at the moment. The characterization of Admiral Alexius Apocaucus as "a rich noble" (p. 154) is a misleading description of the nouveau-riche opportunist, of no ancestral distinction, who rose to influence through the patronage of a real blue-blood, John Cantacuzenus.

But such points are trivial. What matters is that Runciman has produced a concise and very readable sketch of the entire development of the imperial office. It may break little new ground and leave more to be explored, but it is the best, indeed the only, introduction of its kind to the topic to be had.

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ROBERTA C. CHESNUT. *Three Monophysite Christologies: Severus of Antioch, Philoxenus of Mabbug, and Jacob of Sarug*. (Oxford Theological Monographs.) New York: Oxford University Press. 1976. Pp. viii. 158. \$14.75.

This is a work which is both useful and thought-provoking. It is useful because it illuminates a moment in the history of thought which has long been obscure, even when it has not been utterly forgotten. It is thought-provoking because it illustrates admirably how little significance can be attached to ideological platforms, even in the ages which beget them.

The three figures whom the author studies were approximate contemporaries. They were leaders in Syria of the rebellion against the christological orthodoxy established at the Council of Chalcedon (A.D. 451). In opposition to the doctrine of the two natures of Christ as the council had defined it, these thinkers upheld what they took to be the authentic teaching of everyone's mentor, the patriarch Cyril of Alexandria, author of Nestorius' condemnation. Against Nestorius, Cyril had defended the unity of Christ's person by insisting that the one ultimate subject of all Jesus's sayings and doings was the eternal divine Son, and that his human nature, while genuine enough, was not an independent or self-subsistent reality, but rather a condition or attribute of the second person of the Trinity. Severus, Philoxenus, and Jacob asserted and defended a version of this position in their christological teaching.

Roberta Chesnut exhibits a thorough and critical acquaintance with the writings of her three subjects, and provides her readers with useful English versions of a number of significant passages from their works. Even more important, however, is her careful and shaded analysis of their ideas and arguments. As one would expect, part of what this analysis reveals is their dependence on concepts and analogies which had been current in earlier stages of the great christological debate: for example, their use of the analogy of the union of soul and body to explain the unity of godhead and humanity in Christ. What is of more interest, however, is the evidence which Chesnut adduces to establish the originality of her subjects' thought. Severus, for example, has ordinarily been characterized as an exact and systematic expositor of Cyril of Alexandria, and little more than that. Chesnut's analysis shows, on the contrary, that Severus' christology rests on a Platonist analysis of the structure of reality which is, as a matter of fact, quite absent from his master's thought. Severus' christology is, then, the product of a different intellectual agenda from that of its formal original.

It is at this point that the thought-provoking quality of the book emerges; for it is apparent from Chesnut's work that the unity of monophysite thought is purely formal. The religious and philosophical concerns of the three writers studied turn out to be vastly different, not only from those of Cyril of Alexandria in his radical anti-Nestorian phase, but from one another. Philoxenus' understanding of the human situation makes his monophysitism the vehicle of a religious vision quite distinct from that of Severus. It appears in fact that, contrary to all appearances, the technical christological issue which defined the monophysite party was not the central issue in monophysite theologies.

There are few points in this work which require critical questioning. The most obvious, perhaps, is found in Chesnut's treatment of Severus' use of the analogy of "mixture" for the union of divinity and humanity in Christ. Use of that analogy did not necessarily imply a materialistic view of either divinity or humanity; and Severus' employment of the wood/fire image (p. 33) is an admirable instance of just this analogy in one of its many forms. This is a matter of relatively small importance, however, and it certainly does not affect the general argument of a careful, closely argued, and illuminating work.

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JOHN KINNAMOS. *Deeds of John and Manuel Comnenus*. Translated by CHARLES M. BRAND. (The Records of Civilization: Sources and Studies, number 45.) New York: Columbia University Press. 1976. Pp. xii, 274. \$20.00.

John Kinnamos, imperial secretary in the reign of Manuel Comnenus, wrote as an eyewitness to some of the important events of the reign of Manuel and discussed briefly the reign of John, Manuel's father. His is an informed history, particularly useful on topics such as Manuel's relations with Western Europe. That Kinnamos much admired Manuel and left us a particularly sympathetic portrait of him is undoubted, but some of the problems raised by this bias can be corrected by using the work of another major twelfth-century historian, Niketas Choniates.

The Comnenian period is one whose multiple importance for the history of Byzantium and indeed of Europe has long been recognized by scholars. Kinnamos, like Anna Komnene and Choniates, provides information about Byzantine relations with popes, German emperors, and Western kings, with Hungarians, Serbs, and Turks. Internal developments, religious and political, are also described, although less by Kinnamos than by the other two major contemporary sources. These events Kinnamos relates in a language which, although by no means easy, is less pretentious than that of Anna or Niketas, and in a style that is relatively uncomplicated.

It is, perhaps, a measure of the growth of Byzantine studies in English-speaking countries, and particularly in the United States, that English translations of Byzantine sources are increasing in number. Charles M. Brand's translation of Kinnamos is a useful one. His own style is easy and readable; he has subdivided the work into short paragraphs, and has provided headings and an adequate but not overwhelming number of notes.

This makes the work particularly appropriate for quick consultation and for use in the classroom. Of course, those who are doing research in the period will still have to read the original text. For, in important passages, one may have divergent readings from those of Brand. Thus, in the Bonn edition (p. 229), Kinnamos, discussing Frederick Barbarossa's efforts to create an anti-pope, claims that the election of the bishop of Rome is a prerogative of the Roman emperor. In this connection he says that Frederick's actions were caused by his desire to acquire this, the most important mark of imperial authority, i.e., the right to place a bishop on the Roman throne. Professor Brand's translation of the sentence starting "*alla Phredereikos tēi autokratōros palai epophthalmizōn arkhēi . . .*" is, I think, unclear or misleading. Similarly, there is a passage which constitutes an important statement of twelfth-century Byzantine views on the proper relationship between Byzantine emperor, German emperor, and pope. The sentence beginning "*tois de ouk apokhrē monon . . .*" (p. 219, Bonn edition) should, I think, be translated as follows: "for it was not sufficient for them to have usurped the height of Empire as was not meet, having bestowed upon themselves the imperial authority (for this [i.e., *imperium*] means unmixed authority), but they also dare to declare that the Empire at Byzantium is different from the one in Rome." In the notes, Paul Alexander's article, "The Donation of Constantine at Byzantium and Its Earliest Use Against the Western Empire" (*ZRV*, 8 [1963]) should have been cited.

While several such comments could be made, the translation is, on the whole, useful and well presented.

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MICHAEL ANGOLD. *A Byzantine Government in Exile: Government and Society under the Laskarids of Nicaea (1204-1261)*. (Oxford Historical Monographs.) New York: Oxford University Press, 1975. Pp. xx, 332. \$22.50.

When Constantinople fell to the misguided crusaders of 1204, Theodore Lascaris escaped from the city to found a successor "empire" that took its name from its first capital at Nicaea. Under the brilliant leadership of the Lascarid dynasty, the empire of Nicaea outfought its enemies. It was Nicaea's armies, led in 1261 by the usurper Michael Paleologos, that achieved the reconquest of Constantinople.

Was not the reconquest, however, a mistake rather than an achievement? This is the question

implicitly posed in Michael Angold's substantial survey of the institutions of the empire of Nicaea, a book one closes with the wish that the emperors had rested content with their good work in Anatolia and had not pursued the impossible dream of reconstituting a Byzantine empire based upon Constantinople. The author demonstrates that the governmental structure of Nicaea, based upon the imperial household, was tighter and simpler than that of its Byzantine predecessors and thus provided more effectively for the exercise of power. The Lascarids, further, were hard-working monarchs whose constant supervision developed the economy, held the powerful in check, sought to provide equitable justice, and effectively guided the thrust of the armies.

The Nicaean state rested upon a reciprocal compromise between autocracy and aristocracy. In return for the emperor's "friendship," expressed in patronage and grants of rights and revenues in *pronoia*, the magnates rendered service and loyalty. But the usurpation of Michael Paleologos and the recovery of Constantinople fatally jeopardized the compromise. Michael had to grant lands and rights with a free hand to assure his position, and the wide-ranging foreign policy necessitated by the removal to Constantinople distracted later emperors from that minute personal supervision that alone had made the compromise work.

Angold's book so successfully combines a provocative theme with substantial presentation of the evidence that it evokes only respectful comments. A minor point: he argues that the Nicaean empire rested upon a fatally narrow basis of loyalty as rulers and subjects increasingly viewed themselves as "Hellenes" rather than (after the fashion of their ancestors) as "Romans" in a more universal sense. The argument is initially difficult to follow and ultimately unconvincing, particularly since the author so qualifies it as to reduce it to the vanishing point. A final request: Angold is better equipped than most of us to analyze the complexities of policies and practices after 1261. Could he not extend and reinforce his thesis by a survey of the Paleologan period? I eagerly await such a book.

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BERNARD S. BACHRACH. *Early Medieval Jewish Policy in Western Europe*. Minneapolis: University of Minnesota Press, 1977. Pp. x, 213. \$15.00.

The sources for the history of the Jews in early medieval Europe—consisting mainly of laws by Christians—have been discussed and analyzed during the last hundred years by such excellent

historians as H. Graetz, J. Juster, and B. Blumenkrantz, whose investigations have left barely a stone unturned. What is there to add? Bernard Bachrach proposes to inject a new attitude divorced from what Salo W. Baron calls "the lachrymose conception of Jewish history" (pp. vii-x). Thus, according to Bachrach, a closer look at some of the legislation (like that of Recarred I, p. 6) shows it not to be so negative as is usually thought; other laws are no more than defense measures against excessive proselytizing zeal by Jews (pp. 45-46), and Chlotar II's anti-Jewish legislation may be explained by motives of expediency (pp. 53, 60). Yet these examples are not representative of the kind of scholarship this book offers: for the most part Bachrach tells an old-style story of persecution, narrated land by land and in chronological order. Even the anti-Jewish propaganda of Agobard of Lyons in the 820s, whose explanation can be (and has rightly been) sought in the contemporary struggle between state and Church, is presented here (pp. 98-105) as a chapter of medieval anti-Semitism.

A more critical attitude might also have been expected in the treatment of the data pertaining to the Kalonimides family. This is an exceptional lineage of medieval Europe: twelfth-century traditions claimed that the Kalonimides initiated the Jewish settlements in the Rhine Valley as well as the south of France, and this claim was later sustained, quite suspiciously, by all too well-rounded charters attesting to a special relationship between the Kalonimides and Charlemagne. By taking the "legend of Charlemagne" into consideration, Bachrach might have had second thoughts before stating that the family was brought to the Rhine Valley in the ninth century (pp. 71, 86). Not only do none of the many genealogies reach any earlier than the late tenth century, but also Thietmar of Merseburg—in a well-known passage—reports that the Jew Kalonymos saved the life of Otto II in the battle of Cortone in 982. Nineteenth-century scholars already realized that Thietmar's report must be the basis for the later pretension of the family to Carolingian roots (see J. Aronius, *Regesten zur Geschichte der Juden* [1902], p. 58).

Although Carolingian policy toward the Jews occupies the second half of the book, no account is taken of the controversial theory of A. Zuckerman, who argued in *A Jewish Principdom in Feudal France 798-900* (1972) that large parts of southern Gaul and northeastern Spain were under the dominion of a princely Jewish family of Davidic origins (apparently a branch of the aforementioned Kalonimides) whose heirs still lived in Narbonne in 1306. If there is any truth to this thesis—meticulously elaborated with reference to hundreds of sources—then the whole issue of "Jewish policies" must be

viewed from a new perspective; if such a Jewish Atlantis did not exist, then a book on "Jewish policies" ought to explain precisely how Zuckerman went astray. Bachrach, unfortunately, does not take up the glove. He contents himself with a few negative utterances, always too short and buried in the footnotes (pp. 164-65, 181-82, 185-86); it is in another book on Jewish policies in early medieval Europe that we will have to seek an answer.

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ROSAMOND MCKITTERICK. *The Frankish Church and the Carolingian Reforms, 789-895*. (Studies in History.) London: Royal Historical Society. 1977. Pp. xxi, 236. \$12.50.

Although historians have been long aware that the conversion of a pagan people to Christianity entailed an extensive cultural reorientation on the part of the converted population, detailed analyses of such problems, and of responses to them, have been few. That the newly converted populations were often badly informed concerning the particular tenets of the new cult, whether doctrinal or moral, is a familiar proposition infrequently treated in a more than cursory manner. Rosamond McKitterick's monograph on the Carolingian reforms will therefore be a welcome addition to the literature.

McKitterick presents a detailed survey of the Carolingian church reforms and the problems of both the moral and doctrinal instruction of the lower clergy and the lay populations in the Frankish lands in the eighth and ninth centuries. Her sections on the didactic use of the liturgy and on devotional or instructional pieces in the vernacular will be especially appreciated. Her remarks on the Frankish reception of the *Hadrianum*, the attempted reforms of Amalarius of Metz, and the *Florilegia*, will also be appreciated by those who have not specialized in liturgical studies. In general this book seems to be a very sensible survey of the period and the problem. It is gratifying to note that McKitterick has emphasized the importance of ninth-century work in the development of a Christian cultural tradition in medieval Europe, and has placed the religious legislation of Charles the Great in its proper context as a beginning of an achievement, rather than as an achievement in itself.

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REINHART STAATS. *Theologie der Reichskrone: Otto-nische "Renovatio imperii" im Spiegel einer Insignie*.

(Monographien zur Geschichte des Mittelalters, number 13.) Stuttgart: Anton Hiersemann. 1976. Pp. vi, 185. DM 110.

For some eight hundred years, ending in 1806, the octagonal crown of the German emperors remained a distinctive symbol of Western history. The author, in placing the *Reichskrone* in the context of its Ottonian origins, draws together the extensive research of German and Austrian scholarship since 1945. The contributions of Buehlers, Decker-Hauff, Fillitz, and Kurt Fina are, among others, synthesized to provide a rounded and richly detailed account of the crown as an historical artifact.

But if Reinhart Staats proceeds consciously in the distinguished tradition of Percy Schramm in explicating the complex symbolism of Ottonian imperial insignia, he also moves beyond previous approaches to the *Reichskrone*. Of particular interest is the breadth of his investigation, as well as his use of the relatively new discipline of iconology. Staats seeks to grasp not simply the esthetic form of the crown, as in conventional iconography, but above all its substantive spiritual and intellectual content.

Following the iconological method, the author intends to illuminate the character of the Ottonian monarchy by identifying and correlating the main artistic, theological, and political meanings that can be attributed to the crown as a physical symbol. To this end he examines in meticulous detail the religious portraits on the crown, the great cross added to the front of it in the eleventh century, and the precious stones that adorn it on all sides. Four large plates containing the array of gems are joined by hinges to four smaller plates depicting the biblical Kings Solomon, David, and Ezechias and, above all, the Byzantine Pantocrator, the Christ of majesty, the lord of nations. The virtues of wisdom, justice, and long life associated with the three kings, respectively, are then related directly to the ideological and practical needs of the Christian Ottonian monarchy. Thus Otto I and his son Otto II are compared with Solomon and David—a parallel specifically confirmed by contemporary writers such as Hrotswitha of Gandersheim. The Pantocrator can be seen to reflect the high political theology of the dynasty: the Ottonians, like their Byzantine neighbors, regarded themselves as vicars of God on earth and used the imperial crown to proclaim this conviction dramatically. As priest-kings and God's agents they sought the reform or renewal (*renovatio*) of their realm.

Staats is of course not the first to detect such implications in the crown. Again, what is unique about the book is its effort to draw together a

variety of previous traditions (especially Old Testament, Patristic, Byzantine, and Carolingian) to demonstrate how greatly indebted the Ottonians were to the past, even as they added to these legacies in converting them to their political and religious purposes.

Staats subscribes to the view of Karl Bosl that the spirit of the Ottonian era is better sought through its artistic, symbolic manifestations than through its more direct, rational, literary expressions. Such a view goes too far, although the symbolic approach does, when cautiously applied, promise much in supplementing the scant written sources of the period. Staats' book provides a sober, balanced appraisal of the topic that should remain standard for some time to come.

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KENNETH M. SETTON. *The Papacy and the Levant (1204–1571)*. Volume 1, *The Thirteenth and Fourteenth Centuries*. (Memoirs of the American Philosophical Society, number 114.) Philadelphia: The Society. 1976. Pp. ix, 512. \$30.00.

For over thirty years Kenneth Setton has added to our knowledge of Latin involvement in the eastern Mediterranean during the later Middle Ages. He has now published the first of three projected volumes which, when completed, will narrate the history of certain European adventures in the East from the Fourth Crusade to the Battle of Lepanto. This volume covers the period from Innocent III to the early fifteenth century. Despite its title, it is much more and, in certain areas, a bit less than a complete exposition and analysis of papal-Levantine affairs.

Certainly every Roman pontiff who reigned during these two centuries, including the rival claimants of 1378 and following, appears in these pages, and the Eastern policies of several—notably Innocent III, Gregory X, and an Avignonese trio, Clement VI, Innocent VI, and Urban V—are treated in detail. Yet there are entire chapters where the papacy plays, at best, an off-stage role, and full attention centers on some Latin adventurer, such as Henry of Hainaut, Latin Emperor of Constantinople, or Amadeo VI of Savoy, all of whom might, for lack of a better term, be called crusaders. Still this volume is not an exhaustive history of two centuries of crusading warfare. Save for the Fourth Crusade, those armed pilgrimages of the thirteenth century, to which historians have seen fit to assign numbers, are not dealt with. Moreover, some important areas of direct papal involvement in the Levant are not adequately treated. One example is the thirteenth-century papacy's relations with the Church of Cyprus, a

problem that commanded a good deal of papal energy from Honorius III through Alexander IV. To be sure, George Hill's *History of Cyprus* (1948) dealt with the question. But recent publication by the *Pontificia Commissio ad redigendum Codicem Iuris Canonici Orientalis* of later thirteenth-century papal materials relating to the East seems to make a new study of papal-Cypriote relations necessary. Judging from his notes, Setton knew and used these published sources but, in some areas, did not use them as fully as he might have.

What Setton does do, and brilliantly, is to expand significantly upon the themes and subjects of many of his earlier studies. Electing to exclude Cyprus, the Holy Land, and, with the notable exception of Peter I of Cyprus' sack of Alexandria in 1365, Egypt, he presents a series of detailed studies of selected European engagements in the Levant. Those acquainted with his work will not be surprised to find two chapters dealing with Athens under, respectively, Burgundian and then Catalan and Florentine rule. This is not to say that he is rehashing old material. Fully half of this book, that portion devoted to the mid-fourteenth century and beyond, incorporates original, hitherto unpublished research into archival sources.

As is inevitable in work of such ambitious magnitude, there are minor errors of omission and fact. For example, Setton seems not to know of Helmut Roscher's important study, *Papst Innocenz III. und die Kreuzzüge* (1969) or of Gerd Hagedorn's "Papst Innocenz III. und Byzanz am Vorabend des Vierten Kreuzzugs (1198-1203)," (*Ostkirchliche Studien*, 23 [1974]: 3-20, 105-36). He also confuses the reasons why Innocent III rejected the personal appeals of Alexius the Younger with the arguments which the young Greek's uncle, Alexius III, presented to the pope after the fact. But such lapses are, on balance, insignificant.

This is an important work which will stand the test of careful scrutiny by scholars. Indeed, it promises to become the standard in the field for quite a while to come, far superseding Walter Norden's, *Das Papsttum und Byzanz* (1903) and William Miller's *The Latins in the Levant* (1908). We can only eagerly await the appearance of the next two volumes.

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EGBERT TÜRK. *Nugae Curialium: Le règne d'Henri II Plantagenêt (1145-1189) et l'éthique politique*. (Centre de Recherches d'Histoire et de Philologie. Hautes Études Médiévales et Modernes, number 28.) Geneva: Librairie Droz. 1977. Pp. xviii, 233.

Egbert Türk sees the policies of Henry II and his *curiales* as reflecting a new, secular spirit abroad in

the world of twelfth-century Europe. Henry's reign, Türk argues, contributed much to the emergence of the state as an independent entity and to its slow, painful emancipation from the tutelage of the clergy. The *curiales*—a new, mobile, and predominantly secular-minded class—provoked a hostile reaction among rigorous churchmen. Unsympathetic toward the widening split between royal politics and traditional Christian ethics, these old-fashioned moralists focused their attacks on Henry's *curia*, "the symbol of the dangerous spirit of the century" (p. 184). Hence the anticurial bias in the writings of such ecclesiastical sometime-*curiales* as Arnulf of Lisieux, John of Salisbury, Gerald of Wales, Peter of Blois, and (more genially) Walter Map. Several of these men saw their own ambitions thwarted by the *curia*; all were bookish, unrealistic, and committed to an anachronistic interpretation of the state running back to the political Augustinianism of Gregory the Great and his predecessors.

Türk's approach is strongly prosopographical. His sketches of fourteen royal *curiales* depend very heavily, and admittedly, on the doctoral dissertation of J. E. Lally, as does his discussion of royal patronage—much of which is now available in the original in Lally's "Secular Patronage at the Court of Henry II" (*Bulletin of the Institute of Historical Research*, 1976). Türk's sections on Henry's curial critics are less derivative, but the people in question are all well known, and most of the data are easily available elsewhere. The book thus contains little that is new apart from the author's irrepressible anticlericalism, pounded home by an abundance of exclamation marks—as when he speaks of "*la liberté (=privilèges!) de l'église*" (p. 25).

The book is marred by numerous errors. King Stephen's anarchy, for example, is stretched to thirty years (p. 11); Henry II's reign is misdated 1145-89 on both the front cover and the title page, but reported correctly elsewhere. W. L. Warren's standard biography of Henry II (1973) and Knowles' *Thomas Becket* (1970) evidently appeared too recently to have been consulted.

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GÉRARD SIVÉRY. *Structures agraires et vie rurale dans le Hainaut à la fin du Moyen Âge*. Volume 1. Lille: Université de Lille III. 1977. Pp. 343. 65 fr.

The exceptional diversity of agrarian structures in medieval Hainaut is the central theme of Gérard Sivéry's study. The open field plateaux of the Southwest and North had nucleated villages with a classic open three-field structure and enforced

crop rotation. Lords were powerful, often controlling entire villages, while peasants were kept in subjection. This area, the richest and earliest settled in Hainaut, produced grain for export. North-eastern Hainaut had a similar pattern, but fields were larger and sowing patterns were determined by individual peasants, rather than by village organizations or lords. In the East and South near the Ardennes lords also kept extensive rights over their peasants, but were themselves poor. Peasant plots were tiny clearings. Central Hainaut, the Quesnoy-Mauberge-Bavai area and surrounding territories, had gentle hills and valleys, with many small streams and a persistent flood problem. Much of this area was only cleared in the twelfth and thirteenth centuries, and this is the region of most charters of bourgeois liberties. Dispersed settlement and topography meant that central Hainaut was not suited for open field agriculture. Although some pasture was converted to arable in the thirteenth century in response to market demands, there was a return to animal husbandry, with some enclosure, in the late Middle Ages. Crop rotation was usually triennial, but on the individual volition of the farmers. In the fourteenth and fifteenth centuries the bonds of the nucleated village were relaxed and grazing lands were extended. More complex field structures appeared as emphasis switched to animal products and industrial crops. Estate management and accounting became more scientific. Leasing of the demesne came late to Hainaut, around 1250, in a time of prosperity when lords evidently found that such an elastic and periodically renewable arrangement would augment their profits.

Although the nobility was powerful as late as 1200, particularly in northern and central Hainaut, the authority of the counts grew so quickly that by 1300 only the lords of Enghien and Avesnes continued to threaten them. The great lords held numerous allods until around 1280, but the counts had managed to convert most of them into fiefs by the early fourteenth century. Although the feudal vocabulary of Hainaut was quite rigid until the end of the Middle Ages, most lords were plagued by fragmentation of holdings despite theoretically firm rules of primogeniture. Among nobles and peasants alike, older forms of classifying persons, based on birth, gave way to those founded on function, life-style, and income. Nobility in Hainaut was essentially a question of power and life-style; the only absolute essential was possession of a large seigneurie. Nobility was, however, clearly distinguished from knighthood, which was open to anyone prosperous enough to do mounted military service.

It is unfortunate that both volumes of this work were not published simultaneously. The first gives

only a conventional analysis of social classes and field exploitation, topics which others have investigated for Hainaut. Sivéry's extensive use of unpublished sources provides nuances of opinion, but no fundamental departures. While the study of field patterns is carried through the early sixteenth century, the social and economic analysis only goes to 1300. Yet most of Sivéry's sources are from the fourteenth and fifteenth centuries, a period for which his previously published articles have suggested quite novel conclusions. A certain feeling of discontinuity is thus inevitable. It is also absolutely inexcusable that a book of this kind does not include detailed maps of the region.

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JOHN B. FREED. *The Friars and German Society in the Thirteenth Century*. (Mediaeval Academy of America Publications, number 86.) Cambridge, Mass.: The Academy, 1977. Pp. xvi, 280. \$14.00.

The ties between German society and the Franciscan and Dominican Orders are here explored in terms of where and when the friars established convents, who joined the orders, and what role the friars played in society and politics. The expansion of the orders between 1220 and 1250 closely followed the existing urban network of northwestern Germany; the patterns of settlement discerned by John B. Freed match those being found by Jacques Le Goff and his associates in their investigation of urbanization and the mendicant orders in France. The spread of the orders was so systematic that the foundation dates of their convents can serve as an index of the relative size and importance of urban communities. Later in the century, the spread of the friars in eastern Germany was related instead to migration and Germanization; there the friars performed such tasks as preaching to pagans or raising troops for the Teutonic knights. In an appendix the author supplies a directory of German mendicant convents that merits a place alongside the lists established by Knowles and Hadcock for England and Wales and by Emery for France.

The historical and historiographical problems of the social origins of the friars are set out by Freed with admirable clarity. He lists 168 identifiable German friars from the period before 1273 (about 3% of the estimated total membership) and cautiously explains the predominance among them of ministerials and patricians, stressing the likelihood that the sources tell more about the mendicant leadership than the rank and file.

Without moralizing over initial idealism and subsequent decline, Freed traces the history of the

friars' spiritual independence and of its degradation and ultimate compromise. The corrupting element was the papal-imperial conflict, rendered unavoidable for the friars after 1245 by Innocent IV, who turned it into a death struggle with the Hohenstaufen. These three separate matters of geographical and chronological patterns of settlement, social origins of the friars, and social and political involvements of the orders are skillfully woven together as part of a major social and political revolution taking place in thirteenth-century Germany. While at first reacting against social and political change, "collectively and consciously," concludes Freed "the friars came to admire and ultimately to promote the transformation of the traditional social and political order" (p. 170).

The social history of medieval spirituality is thus greatly enhanced by this intelligent and thorough book. Moreover, the outlook continues to be favorable, to cite only the recent Yale dissertation of Frederick M. Stein on "The Religious Women of Cologne, 1120-1320." Stein, like Freed (who devotes an entire chapter to the friars at Cologne), acknowledges a considerable debt to Herbert Grundmann, and also, like Freed, strives to compensate for the considerable deficiencies in Grundmann's social analysis.

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MODERN EUROPE

WALTER ULLMANN. *Medieval Foundations of Renaissance Humanism*. Ithaca, N.Y.: Cornell University Press. 1977. Pp. xii, 212. \$12.50.

Baptismal rebirth is the key, Walter Ullmann argues in this provocative study of the nature and origin of Renaissance humanism, to unlocking the fundamental outlooks of both the Renaissance and the Middle Ages. Medieval aspirations, articulated first in the Carolingian Renaissance, developed the ecclesiological implications of the baptismal transformation of man's being in an attempt to create a wholly reborn society in which all public and private life was subsumed under Christian norms. Renaissance humanism reversed this process by giving "rebirth" to man's unregenerate nature and thereby restoring to him the social and political autonomy denied by the theocratic thrust of medieval society.

Just as the nature of Renaissance humanism is inexplicable without regard to the baptismal theology of the Middle Ages, so its origin also must be sought in medieval developments. Indeed the ideological defense of secular government prompted by

the investiture contest is the matrix of humanist thought, and the catalyst in the process is recourse to Roman law. Granted, the twelfth century was also marked by other secular forces such as the expansion of trade and the growth of towns, but Roman law restored the crucial link to the classical world. Secularization of government entailed the gradual emergence of the concept of man as citizen. This was achieved in the thirteenth century through the reappropriation of Aristotelian thought and the new "political science" of Thomas Aquinas. No fundamental break, however, in Ullmann's view, separates the fifteenth from the twelfth century, and the literary, cultural, philosophic, esthetic, and educational aspects of humanism, often seen as its essential features, should rather be regarded as sequels to the prior, and more critical, creation of a citizen-centered culture.

Ullmann's thesis, which reveals the proclivities of much of his previous scholarship—in particular the centrality of law in defining the essential features of any society, and the fundamental importance of Roman law for medieval legal and political theories—has the merit of clarity, succinctness, and rigor. Certain aspects, however, remain troubling. To relegate the nonpolitical concerns of humanism to the status of "epiphenomena" ignores the Renaissance search for the meaning of selfhood, a vital matter to Petrarch and subsequent humanists. (Indeed for Ullmann, Cola di Rienzo is a more significant figure than Petrarch.) Similarly excluded is any attention to the revival of rhetoric, seen in much recent scholarship as a defining characteristic of Renaissance humanism. Finally, to argue that fifteenth-century England remained unreceptive to the cultural aspects of humanism because the dominance of feudalism there involved a "precitizen capacity" which required no rehabilitation of natural man suggests that the Kingdom of Naples, the most feudalized part of Italy, should have been at the forefront of the Italian Renaissance instead of the city-states of Northern Italy.

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THOMAS N. TENTLER. *Sin and Confession on the Eve of the Reformation*. Princeton: Princeton University Press. 1977. Pp. xxiv, 395. \$25.00.

Henry C. Lea's *A History of Auricular Confession and Indulgences in the Latin Church* (3 vols.) was published more than eighty years ago. Its bias has been well known, but there has been no replacement. Thomas N. Tentler's *Sin and Confession on the*

Eve of the Reformation does not purport to be that replacement. This book is, however, the kind of scholarly research that will make possible a new and complete history of the sacrament of penance. Tentler's scope is more restricted than such a history would be, but the excellence of his scholarship in his chosen period of research makes this work a model for anyone who would investigate other periods in the history of confession.

Tentler's concern is to communicate the theory and practice of the institution of penance and its related topics of sin, guilt, and forgiveness in the decades preceding the Reformation. His principal sources are books on confession printed in these years, especially those treatises intended for the formation of confessors. Tentler's knowledge of these complex and verbose works is thorough; he has not only a firm grasp of the information but also a profound knowledge of the meaning of these texts and contexts. He moves with ease through the intricacies of a very foreign culture. The distinctions that abound in these works are no barrier to Tentler's clear presentation of the import of nuances that would seem to be mere hairsplitting to a less knowledgeable and experienced investigator.

The author places the understanding of confession during the decades before the Reformation in perspective with a clear and concise survey of its historical development from public penance in the early Church to a private ritual in the Middle Ages. The chapters on the content and meaning of themes of penance in the period before the Reformation show us the author at his best. Throughout the book the footnotes are extensive and helpful. The bibliography, however, is selective rather than exhaustive.

Tentler unifies his research about confession under the themes of discipline and consolation. Since he is more interested in presenting the "social and psychological impact of the institution" (p. xi) than in exploring theological ideas, these themes can be appreciated as necessary organizational tools for his presentation. He has, however, confined his outlook too much by making discipline equivalent to social control. Undoubtedly the institution of confession has been a means of clerical control (not always considered a pejorative term by Tentler); yet, there are devotional, spiritual, and theological ramifications to penance that call for elucidation, and which go beyond the notion of social control and the idea of consolation. In fairness to Tentler one should also read his contribution on this notion of social control in Charles Trinkhaus, ed., *The Pursuit of Holiness in Late Medieval and Renaissance Religion* (1974).

Tentler's interesting treatment of sexuality in the confessional gives the non-specialist some idea

of the restrictive religious view of sex in the Middle Ages, an outlook that needs to be complemented from other sources.

Sin and Confession is the definitive work of a mature scholar. No replacement will be needed. It is thorough, balanced, and very sensitive to the complex and often emotionally charged issues surrounding the sacrament of confession on the eve of the Reformation. The publisher has produced a carefully printed book. On page 189 the word century is omitted and the index could have been more complete, but these are inconsequential flaws in a volume that is both scholarly and humane.

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KENNETH R. STOW. *Catholic Thought and Papal Jewry Policy, 1555-1593*. (Studies in Jewish History, Literature and Thought, number 5.) New York: Jewish Theological Seminary of America. 1977. Pp. xxviii, 411. \$25.00.

Historians have previously considered the *Talmud* burning in Rome in 1553 and the issuance of the anti-Jewish papal bull, *Cum nimis absurdum*, in 1555, as parts of a threatened papacy's overall strategy to safeguard Catholic orthodoxy by subjugating all dissident groups and by erecting barriers between them and faithful adherents of the "true" Christian faith.

Kenneth Stow, in his new work, proposes a different interpretation of these disruptive measures: their primary purpose was the mass conversion of Jews. Prior to 1553, the papacy never actively sought large-scale Jewish conversion but spoke of it only as a distant goal. The late sixteenth-century papacy, however, undertook a multifaceted program of severe economic, political, and religious restrictions designed to encourage immediate conversion.

For Stow, a key to understanding the new papal policy is the legal tract, *De Iudaeis et Aliis Infidelibus* of Marquardus de Susannis, published in Venice in 1558. This composition represents an excellent summary of earlier legal opinions about Jews but also "through the *De Iudaeis*, the policies of Paul IV and his successors may be seen unerringly" (p. xiii). By defining carefully Jewish legal status in Christian society and by arguing for the rigorous application of the law, de Susannis' tract mirrored the hope of contemporary popes and Catholic thinkers to lead Jews to the baptismal font.

Stow's exhaustive study is an impressive and persuasively argued work; his careful analysis of the *De Iudaeis* is a model of textual explication. Moreover, his ability to utilize the text to reduce

the complexities of sixteenth-century papal Jewry policy to a single motivating factor is particularly notable. But perhaps the apparent virtue of such a uniform explanation might also reveal a flaw in Stow's overall argument, that of occasional overstatement.

Stow vigorously marshals abundant evidence, for example, to explain how Paul's policies represented a watershed in Christian treatment of Jews. But much of this evidence is also applicable to the period preceding Paul's papacy. A clearly discernible community of thought in favor of active conversion was apparent long before the 1550s, and had been energized by intense eschatological speculation, the threat of Turks and Protestants, the new discoveries, and Christian kabbalistic study. Even de Susannis' insistence that all Jewish internal jurisdiction be removed to facilitate Jewish conversion was hardly a new idea but instead a constant reality of Italian Jewish life for generations before, as R. Bonfil has recently shown. In fact, the cumulative effect of Stow's evidence suggests that rather than a watershed, Paul's policy was, to a large extent, merely a natural culmination of trends already discernible decades earlier.

Stow also tends to exaggerate the impact of Paul's policy on Italian Jewry. Undoubtedly some Jews converted, but the Jewish community was never uprooted. On the contrary, Italian Jewish culture continued to flourish, and contemporary Jewish writing, notwithstanding Stow's assessment, did not necessarily reflect despair and pessimism (p. xvii). Many of the earlier Renaissance trends persisted, combined with a renewed interest in mysticism, unrelated directly to papal restrictions. Perhaps Jews commented infrequently on the specific changes in papal policy not only because of their self-imposed censorship, as Stow suggests, but because they often failed to discern the momentous shift in that policy in the first place, despite the deteriorating conditions of their lives.

Yet such matters of emphasis are relatively minor in evaluating Stow's book. The author has made an important contribution to our understanding of papal Jewry policy. Moreover, whether incidental or not, Stow has skillfully illuminated from a perspective previously neglected the reaction of a shaken papacy to the cataclysmic changes affecting the entire Christian world in the sixteenth century.

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SYDNEY ANGLO, editor. *The Damned Art: Essays in the Literature of Witchcraft*. Boston: Routledge and Kegan Paul. 1977. Pp. viii, 258. \$16.50.

In recent years the most influential authorities on witchcraft in early modern Europe have interpreted the witch-craze of the sixteenth and seventeenth centuries as an issue in the history of popular culture and have looked to the social sciences for methodological guidance. Important intellectual-historical studies of medieval witchcraft have appeared, but the history of the idea of witchcraft in the early modern period remains to be written. The author of that history will be grateful for the very useful collection of essays which Sydney Anglo has given us. Though most of the writers studied in Anglo's volume worked in the sixteenth century, the chronological limits of the collection, 1476 and 1705, are far enough apart to accommodate a real variety of authorship: Heinrich Kramer and Jacob Sprenger, Gianfrancesco Pico della Mirandola, Johann Weyer, Jean Bodin, Reginald Scot, George Gifford, James I, Pierre De Lancre, Cotton Mather, and John Bell. The ten essays in *The Damned Art* are, for the most part, informative introductions to significant witchcraft tracts. Had the individual essays appeared as preliminary material in editions of primary works, most might have gone unnoticed, but collected they become a chorus of insistent questions about our understanding of early modern witchcraft. Specialists may find that too much space is given to outlines and précis of well-known sources, but they will also find some novel and convincing lines of inquiry.

Peter Burke on Pico's *Strix* and Anglo on the *Malleus maleficarum* argue that the particular problem of witchcraft needs to be examined in the wider context of the revival of occultism in the later Middle Ages and after. Christopher Baxter's study of Bodin is a convincing application of this argument. Baxter shows that Bodin's idiosyncratic Judaism makes sense both of his harsh views on witchcraft and of his criticisms of the Neoplatonic magic of Ficino and his imitators. Anglo's second contribution to the collection, an analysis of Scot's *Discoverie of Witchcraft*, demonstrates that Scot too was influenced by contemporary Hebraic scholarship. That Scot's skeptical answers to the witchcraft question were unusual, not to say anomalous, in his time seems clear from Anglo's presentation of the evidence. One can fault Anglo only for having been too modest in putting the case for Scot's *Discoverie* as a genuine turning-point in the history of witch-beliefs. Scot was willing to challenge some of the most fundamental articles of Christian teaching; Anglo attributes his boldness to his having been an "... independent-minded country gentleman ... neither a theologian, philosopher, nor magus." Alan Macfarlane's analysis of George Gifford's witchcraft treatises affords an apt contrast. Gifford, a Puritan divine, was humane and

generally suspicious of the witch-craze, yet his religion and his profession kept him committed to some measure of belief in witchcraft. Gifford's tracts sound traditional when compared to Scot's *Discoverie* but innovative when read against the *Daemonologie* of James I. Stuart Clark shows us that the context of the *Daemonologie* is James' monarchism and makes some tantalizing suggestions about witchcraft as an idea of disorder in dialectic with the orderly hierarchies which James and others wished to maintain and promulgate. Christina Lerner's account of two Scottish witchcraft treatises from 1697 and 1705 and Margaret McGowan's description of Lancre's *Tableau de l'inconstance des mauvais anges et demons* are less ambitious than most of the other essays, but worth the reader's attention. *The Damned Art* fills a real gap in modern witchcraft studies, and one hopes that as useful a book will be written on the very important and badly understood witchcraft literature of the fifteenth century.

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JOSÉ ANTONIO FERRER BENIMELI. *Masonería, Iglesia e Ilustración*. Volume 1, *Las bases de un conflicto (1700-1739)*; volume 2, *Inquisición: Procesos históricos (1739-1750)*. (Publicaciones de la Fundación Universitaria Española. Monografías, number 17.) Madrid: Fundación Universitaria Española. 1976. Pp. 440; 546.

These massive tomes, to be followed by two more carrying the story to 1800, are truly a labor of love. The author, a historian at Saragossa, has done four previous masonic studies and two on the Conde de Aranda. He combed every relevant archive, from the Vatican outwards, and approximately half of these pages are documentary appendixes. Perhaps the key to the work lies in the subtitle in each volume: "*Un conflicto ideológico-político-religioso*."

Ferrer Benimeli believes that it is high time for a dispassionate discussion of eighteenth-century masonry, its supporters, and its enemies. He traces the evolution of the order (or fraternity) from its medieval guild origins, whose architectural-engineering character was to determine much of later masonic language and ritual. In that discussion Ferrer describes and discards the more legendary notions of the genesis of freemasonry, some of which dated it to various ancient societies. What precisely caused the late seventeenth- and early eighteenth-century emergence of the non-professional, gentlemanly freemason, initially in Scotland and England, is not at all clear, but certainly by the 1720s and 30s when formal lodges were instituted and spread to the Continent their

basic characteristics were evident enough to those who feared them. At first their secrecy and ritual attracted much suspicion from Protestant and Catholic alike; that freemasons stood for equality within the order and religious toleration in general was not compensated for by early masonic oaths and charters which repudiated atheism and libertinism and recommended that masons avoid challenging the religious status quo. A clear indication of masonic principle was the election of a leading Catholic aristocrat as English Grand Master in 1729.

In fact Ferrer's evidence suggests that Englishmen resident in Catholic lands were the usual founding fathers of Continental lodges, joined subsequently by Frenchmen. Masonry attracted many titled persons such as Francis of Lorraine, subsequent Grand Duke of Tuscany and husband of the Empress Maria Teresa. Typical Spanish reactions to masonry in these formative years were to view it as "atheistical, Jewish, foreign, and Pythagorean." The papacy was horrified by what it understood of the movement and Clement XII formally condemned it in the bull *In eminenti* in April 1738. Ecclesiastical and secular authorities alike distrusted it as a secret sect endangering all institutions. (Although the author does not say so, I believe this attitude had much in common with the contemporary one toward Jesuits, despite some obvious differences.)

Much of volume two is devoted to the spectacular Grudeli case in Florence (1739-41), in which the Tuscan Inquisition hounded the leading mason literally to his grave, despite retractions from initially hostile witnesses and the efforts of the Grand Duke Francis to intercede on his behalf. As a result of this episode Benedict XII transferred the head inquisitor and conceded tribunal representation to ducal appointees; thus the Venetian model of having state watchdogs on a regional inquisition came to Tuscany. Nonetheless, Francis was unable to save his fellow Viennese masons from a government "raid" and arrest at his wife's orders in 1743; the pope, however, consented to the detained being absolved by priests.

Obviously there is much in these tomes which space forces a reviewer to omit. On the other hand I am constrained to conclude that Ferrer would have benefited from editorial advice. The immensity of the detail, particularly as supplemented by the voluminous appendixes, is not necessary; much room for intelligent pruning exists. Certainly this is not to deny that the work enormously enlarges our knowledge of eighteenth-century freemasonry and particularly of Catholic responses to the phenomenon. It seems a curious parallel to our day, though, when one reads how many sincere Catholics, including clergy, quietly ignored the

papacy to become masons, as many apparently do now in other respects. Finally one misses the analytical acuity of, say, Robert Darnton, when Ferrer Benimeli alludes only vaguely to deviations within the order, especially in the 1770s and 80s. But this is symptomatic of the work's other major flaw, the author's inability to rise above his mass of data to synthesize excitingly, as the subject demands. In fairness, however, we must await the concluding volumes in this, as in other, respects.

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ÉMILE POULAT. *Église contre bourgeoisie: Introduction au devenir du catholicisme actuel.* (Religion et Sociétés.) Paris: Casterman. 1977. Pp. 290.

Émile Poulat intended this book to be a semi-popular study of certain issues raised, but not discussed, in his *Catholicisme, Démocratie et Socialisme* (1977). Because he focuses primarily on the present and future in this work, he employs sociological more than historical methods, and, consequently, the book should be judged by the standards of both disciplines.

In this book, Poulat indicates the general direction in which he believes Catholicism (particularly French Catholicism) will develop in the near future. He briefly (and, as a historian, I would say most inadequately) sketches the origin of current Catholic problems. He belatedly justifies his brevity (p. 213) by denying to modern Catholicism any unilinear development from the past, asserting that various modern Catholic problems are examples of "polygenesis."

The author's historical thesis strikes a historian raised outside, but familiar with, the French Roman Catholic tradition as pedestrian. He attacks a "straw man" never accepted by professional historians—the image of a Catholic Church which is the tool of entrenched society and hence of its dominant class, the bourgeoisie. Poulat argues instead that the Catholic Church always has had more in common with any or all anticapitalist or antiliberal movements than with the liberal bourgeoisie.

I am not equipped to judge whether Poulat's sociology is better than his history. I can make the following observations, however: the author repeatedly insists that no dualistic analysis fits contemporary history, particularly church history. Instead he proposes a triangular model consisting of right, left, and a nebulous political third force, or alternatively, of modernistic, integral, and fundamentalist Christianity. Poulat believes (or perhaps, hopes) that future Christianity will develop a third force based on integral Catholicism.

The terms that Poulat uses require careful definition; that he is aware of this need is evident in his seventh chapter where he discusses the tendency of modern scholarship to devote considerable space to linguistic questions. But his use of highly metaphorical language violates his own strictures and makes his work excruciating for any conscientious reader to follow. Indeed, at first glance, one is impressed by the facility and richness of Poulat's style until one begins to question its appropriateness. Does his wealth of metaphor contribute to or detract from the reader's comprehension by evoking images not necessarily intended by the author (see pp. 30, 100, 213)? Metaphors have their place in narrative, descriptive writing, but they are dangerous in analytical prose. Consequently, I found myself rereading and pondering paragraphs to an extent which I believe the author of a nonscholarly work cannot expect.

This work fails on both the scholarly and the popular level. It adds little to the specialist's understanding of the genesis of the problems currently confronting the Catholic Church, while, I fear, it does little to illuminate for the nonspecialist contemporary thought on this subject.

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G. TEITLER. *The Genesis of the Professional Officers' Corps.* (Sage Series on Armed Forces and Society, number 11.) Beverly Hills, Calif.: Sage Publications. 1977. Pp. 246. \$13.50.

In this essay, a translation from the 1974 Dutch edition, a social scientist at Erasmus University, Rotterdam, attempts to trace the development of the professional cadre of army and naval officers from the viewpoint of sociological theory. The first part of the book, devoted to problems of classification and definition, is the most original. A professional corps must have three characteristics: 1.) the monopoly of certain technical skills that are interchangeable among professionals but which cannot be performed by amateurs, 2.) a sense of esprit de corps rooted in a distinct life style, 3.) the ethos of service to the state. Before such professional corps can emerge, their societies must have developed to a point where the tactical military problems and their solutions can be standardized and thus predicted, the state must be reasonably powerful with an effective "monopoly of violence," and there must exist an "interplay" of social groups, aristocratic and bourgeois, which contribute to the technical and esprit de corps elements. Teitler finds such characteristics most likely to occur in societies which indulge in "instrumental" warfare, rational and limited in objectives—unlike

"absolute" war based upon destruction of the enemy or "agonistic" war with its medieval concept of deeds of valor and code of honor.

Given these considerations, Teitler poses the question: when, where, and why did military leadership assume professional characteristics? He sees such professionalization occurring first in the British navy, culminating in the eighteenth century. In England, rather than in its maritime rivals France and the Netherlands, the proper degree of state centralization and the interaction of aristocratic and bourgeois classes evolved. In the army, mature professionalization first developed in nineteenth-century Prussia with its interplay of an aristocratic officer class and the strong technological emphasis of the general staff. Such extreme professionalization is not without danger, Teitler warns: a professional can be so overspecialized that he becomes arrogant and ignores political, economic, psychological, and ideological factors. The military professional today runs the risk of becoming a new "aristocrat"—awaiting, no doubt, the next movement of the dialectical process.

This summary does not do justice to Teitler's efforts; his essay is a much more sophisticated "think piece" weaving together technological, social, and political elements. A historian, however, may find much to quibble about: the strong underpinning of a dialectical process, the broad generalizations, the "sociological jargon" so fashionably deplored by many historians. Moreover, his sources are entirely secondary, mostly general studies. For example, he uses Lacour-Gayet, Roncière, and Chevalier on the French navy, but ignores a number of specialized naval studies, particularly in periodical literature. Teitler does not use standard French military histories of the *ancien régime*, such as those of Tuetey or André, or the recent work of Corvisier—the leading French military historian of this period. His discussion of centralization in France is based largely upon American studies summarizing French authorities. It is unbelievable that one can write about government in early modern France without mentioning the name of Roland Mousnier. In addition, Teitler has the annoying habit of citing sources in scientific style, by author and year, usually lumping many authorities in a single footnote without reference to page numbers. But these are the drawbacks of being a sociologist rather than a historian; the essay remains an intelligent attempt to answer the important question: how did military officers develop into a professional class.

Sage Publications, however, have done Teitler a strong disservice. There were well over one hundred misspellings, joined words, and other typographical errors—enough to justify submission to *The Guinness Book of World Records*. A good editor

would have eliminated these annoying distractions.

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CHRISTOPHER DUFFY. *Austerlitz, 1805*. Hamden, Conn.: Archon Books, 1977. Pp. xi, 194. \$14.50.

On December 2, 1805, Napoleon won what many consider his greatest battle, defeating the combined Russo-Austrian forces at Austerlitz. Outnumbered, ill-supplied, and concentrated by forced marches, Napoleon's new instrument, the *Grande Armée*, crowned its master's lightning campaign with a tactical and strategic victory. The Russians were driven back into Poland, the Third Coalition was ruined, and the Holy Roman Empire came to an end. The momentous results and the drama of the battle, fought in the presence of three emperors, have been described and analyzed many times, by participants ranging from Sergeant Coignet of the Imperial Guard Grenadiers to General Kutuzov, by military historians, and by literary giants such as Tolstoy. Nonetheless, the present volume is welcome, not only because it has the merit of brevity and readability, but also because it provides a balanced picture both of the victors and the vanquished.

Christopher Duffy, senior lecturer at the Royal Military Academy, Sandhurst, has already established a solid reputation with his works on the army of Frederick II and Maria Theresa and has demonstrated his grasp of Napoleonic warfare in his well-received *Borodino and the War of 1812* (1973). The present battle study follows the broad outlines of the previous work. Duffy provides short, but informative, sketches of the strategic plans and the opposing armies, as well as useful data on contemporary artillery, equipment, and battle tactics. He continues with a brief description of Napoleon's famous maneuver on the rear, the march from the Rhine to the Danube trapping the "unfortunate Mack" at Ulm, and then follows up with the seizure of the Danube bridges, the capture of Vienna, and the attempts to force Kutuzov into premature combat. About one half of the book is devoted to a vivid and lucid description of the battle, in which both sides displayed considerable gallantry though neither Russian nor Austrian generalship could equal that of Napoleon at the zenith of his powers.

Based on a wide range of sources, printed as well as manuscript, including the little-known accounts of British liaison officers present, after-action reports by Austrian officers, and the memoranda composed by Kutuzov, the book is also well supplied with tables, maps, and illustrations, including photographs of the field and of major objec-

tives in their present state. Altogether this volume is recommended not only to the military historian and the armchair general, but to all students of the Napoleonic era.

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HENRYK WERESZYCKI. *Koniec sojuszu trzech cesarzy* [End of the Three Emperors' League]. Summary in German. Warsaw: PWN. 1977. Pp. 408. 58 Zł.

This concluding volume of Henryk Wereszycki's three-volume history of the Three Emperors' League crowns a career that has spanned more than a half century of distinguished scholarship on nineteenth-century Polish and European politics by one of Poland's most respected modern historians. In reconstructing the relations between the Habsburg, Russian, and German empires in the two decades after Sadowa, Wereszycki demonstrates how the Polish question influenced relations between the partitioning powers, even after the French defeat at Sedan, when it ceased to be a European issue. The continuing significance of the Polish question, Wereszycki maintains, is often overlooked in writings on the period of the League. Although Bismarck's diplomacy occupies the center stage, Wereszycki's account is largely from the vantage point of Vienna. Austria-Hungary's correspondence with its representatives in Berlin and St. Petersburg comprises Wereszycki's principal source, supplemented by the microfilmed files of the German Foreign Office in London's Public Record Office.

In the first volume, on the diplomatic background to the League (*Sojusz trzech cesarzy: Geneza* [1965]), Wereszycki showed how Bismarck adeptly exaggerated and exploited the specter of Polish insurrection to assure Russia's neutrality in 1870. By 1872, with Bismarck's prodding, mistrust between Vienna and St. Petersburg was overcome; Austria acquiesced by circumscribing Polish "autonomy" in Galicia, and Russia agreed to dampen Pan-Slav agitation among the Austrian Slavs.

Wereszycki's second volume (*Walka o pokój europejski* [1971]) covered the peak years of Bismarck's diplomatic mastery, from the League's establishment in 1872-73 to the Congress of Berlin. Even at this time of relative accord between the three empires, Bismarck still occasionally invoked the "bogey" of Polish insurrection to elicit Austrian and Russian backing for his anti-French and anti-Vatican schemes.

This concluding volume treats the period from the Berlin Congress to the lapse of the League agreement in 1887. Already at the Berlin Congress, Russia's bitterness toward its "unfaithful" friend

Germany—over the dismantling of tsarist gains in the war with the Turks—overshadowed even St. Petersburg's resentment toward her British and Austrian rivals in the Balkans. In 1879 Bismarck's creation of an alliance with Austria-Hungary again awakened the Polish question. (Wereszycki argues that throughout the 1880s the Polish issue surfaced in direct proportion to the rumors of an Austro-Russian conflict.) Russian publicists quickly raised the possibility of reaching an understanding with the Poles, and Franz Joseph hurriedly visited Galicia in 1880 to demonstrate the Poles' attachment to Austria. Bismarck used the Polish trump card to maneuver Russia into reestablishing the League in 1881. But when a Balkan crisis erupted anew in 1885-87, Bismarck's anti-Polish statements and measures failed on this occasion to improve relations with St. Petersburg; in fact they caused considerable embarrassment in Vienna. No maneuvering of the Polish issue now seemed capable of diverting Austro-Russian rivalry in the Balkans.

In Wereszycki's view, the League never functioned effectively after Russia's imposition of the Treaty of San Stefano. The "second" Three Emperors' League was in reality a fiction; the documents of 1881 and 1884 lacked even the monarchs' signatures. Although the prospect for Poland's rebirth was seemingly nonexistent around 1887, Wereszycki concludes that the rupture between Berlin and St. Petersburg removed the most important obstacle to eventual Polish independence. But not until 1916 would the Polish question again preoccupy the policy makers in Europe's chancelleries.

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WILLIAM ROGER LOUIS. *Imperialism: The Robinson and Gallagher Controversy*. (Modern Scholarship on European History.) New York: New Viewpoints. 1976. Pp. 252. \$12.95.

The most fundamental contribution to the study of imperialism made in recent times is that offered by John Gallagher of Cambridge University and Ronald Robinson of Oxford University. This contribution has grown and been modified through their work, first in an influential article, "The Imperialism of Free Trade," in 1953; then in a major volume, *Africa and the Victorians*, in 1960; and then with important extensions of their argument in their chapter in the *New Cambridge Modern History* in 1962. Their names have joined those of such scholars as Frederick Jackson Turner and Charles Beard, R. H. Tawney and Arnold Toynbee, Karl Marx and André Gunder Frank, in having offered

a "thesis" which is presumed to be summarized upon the invoking of those names.

Robinson and Gallagher gave rise to a substantial body of revisionist work, resting upon their fundamental contentions: 1.) that the New Imperialism of the 1870s marked no significant break with the past, as usually argued, but showed marked continuity with that which went before; 2.) that free trade, rather than formal annexation, helped promote an informal empire which was as tenacious as any created by troops and treaties; 3.) that the essential dynamic which drew European powers, often reluctantly, into formal empire arose from African and Asian events which were the products of the indigenous societies, so that one cannot expect to understand imperialism without a close study of those societies; 4.) that imperialism worked most subtly and best through a matrix of collaborators which developed in reference to local, indigenous needs and on the basis of traditional (and later, newly emergent) local elites; and thus 5.) that the Marxist interpretation of imperialism was simplistic, frequently wrong, and at best useful only at certain times and in certain places.

While scholars of British and, to a lesser extent, other European imperialisms soon came to grips with these analyses, either to accept them and apply them to case studies or to reject one or more of them, students of American imperialism and of Latin American dependency theory have remained surprisingly unaware of the ways in which the Robinson and Gallagher thesis—whether correct or not—could be helpfully used in their own inquiries. William Roger Louis has performed a valuable task, therefore, in making both the general argument and the range of possible objections to it available to a wider audience. He has written a fine, and lengthy, introductory essay which ably summarizes the contributions made by Robinson and Gallagher, as well as the objections which may most reasonably be raised to their analyses. To these he has joined three key selections from their own work, followed by three selections from the contemporary critical response. He has then asked nine scholars to prepare short, new appraisals of their response to Robinson and Gallagher. The result is a book of great value to the scholar who wishes quickly to draw abreast of the controversy as well as to the undergraduate who requires an able summary of the state of imperial studies in the last quarter-century.

Not all contributors have served Louis equally well. A. J. P. Taylor and W. W. Rostow do not address themselves to the central issues, and Ernest R. May provides only the briefest of bibliographical notes, which does not reveal any clear influence that Robinson and Gallagher have had

on American scholarship. Henri Brunschwig and Hans-Ulrich Wehler, on the other hand, show that French and German scholars have absorbed (if only to reject) the main contentions, and Sydney Kanya-Forstner provides a genuinely new set of insights in his commentary, as do Roger Owen, Richard Graham, and Akira Iriye, although to a lesser extent. The book concludes with a succinct statement by Louis on how the work of David Fieldhouse fits into the controversy.

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HOWARD ZEHR. *Crime and the Development of Modern Society: Patterns of Criminality in Nineteenth-Century Germany and France*. Totowa, N.J.: Rowman and Littlefield. 1977. Pp. 188. \$15.00.

In this moderate-sized book Howard Zehr has sketched the large vista of crime in nineteenth-century Germany and France. The book is divided into an introduction about theory, a study of property crimes, and a study of crimes of violence. These chapters give evidence of Zehr's common-sense attitude to the study of crime in a historical perspective. His approach has its charm and its limitations. The author's theoretical discussion is the weakest. He briefly discusses several theories about crime: 1.) that crime is the result of social disorganization; 2.) that crime results from frustration in a situation of rising aspirations (the theory of relative deprivation); 3.) that crime is a form of social conflict. This discussion is not very explicit and pays no attention to the psychological motivation of crime *in connection with* changing circumstances.

In his chapter on property crimes, particularly theft, he throws fresh light on the well-worn subject of the positive correlation between the price of food staples and the level of thefts. Up until 1870 the correlation holds (though he pays no attention to the fact that the "hungry forties" may unduly influence his positive score). After 1870 the correlation disappears, but there is a firm suggestion that the level of thefts becomes linked to the business cycle. The same can be said of crimes such as assault and homicide. There is an indication of a negative correlation between crimes of violence and food staples, meaning that better times gave rise to greater violence. In the later nineteenth century there may be a positive relation between the business cycle and crimes of violence. Zehr's analysis opens an important alley of speculation. What we witness in these facts, according to Zehr, is the transition from a premodern to a modern pattern of crime. In the premodern period, crime was the expression of poverty or sudden (relative) abundance for poor people; in modern times,

crime has become linked to rising expectations, and to frustration created by the blocking of access to abundance. Not only can the cycles of theft and violence be explained in this way, but also the rising level of theft and violence (though homicide went down) during the nineteenth century.

Zehr also tries to answer the question of whether the process of urbanization had any influence on theft and violence. He finds little evidence for this. True, levels of theft were higher in urban than in rural areas—particularly in the first half of the century, and particularly in France—but the growth of cities does not seem to influence the level of crime. This would perhaps have been a rather easy conclusion (for who still believes that the cities were particularly wicked places?) if J. J. Tobias had not made an ill-judged but popular attempt to give this expression of conventional wisdom a new lease on life in his *Urban Crime in Victorian England* (1972).

Zehr is modest in his claims for the statistics he uses, though I wish he had given us his opinion on the hazards of collection and the criteria used, with which anyone who uses statistics published in the nineteenth century must cope. Published statistics are usually so unbelievably bad that these hazards should be critically examined.

Zehr writes: "Macro-analysis, though in many ways a precondition for any closer study, is not adequate to deal with . . . issues with any degree of precision" (p. 137). The omission of "such" in this sentence gives Zehr's meaning a more general touch, but I think he would agree that case studies would give body to his approach, which computes correlations between black boxes, the contents of which are not known. Not only case studies, but another type of material is needed for further analysis. Crime statistics easily create the naive impression that crime is a fact independent in itself. No so. To paraphrase a famous simile by Marshall: Crime and the judicial process are like two blades of a pair of scissors. Neither can be defined without the other. When one blade moves, we say the crime rate goes up, but the other blade, the judge's verdict, not only influences crime, but in some way defines it, and vice versa. That is to say, we should also turn to the original court records, where we can catch a glimpse of the criminal (and the judge) as an individual. Nor are statistics discarded in this way, for we can compile them from the original records, and the reliability of the data depends on our own handling of them.

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MÁRIA ORMOS and MIKLÓS INCZE. *Európai fasiszmusok, 1919–1939* [European Fascisms, 1919–1939]. Budapest: Kossuth Könyvkiadó. 1976. Pp. 309. 30 Ft.

According to the authors of this work, the study of fascisms is experiencing a veritable renaissance among Western historians. After reading this book by Mária Ormos and Miklós Incze Western historians can answer that the factual, objective study of Fascism in the Eastern Bloc can now celebrate its veritable nativity! And this has occurred in Hungary, where a relatively objective approach to the study of native Fascism paved the road to this comprehensive study of all European fascist movements, a study which attempts to explain the fascist phenomenon as such, and its place in history and in society—complete with a selective but large bibliography of works relating to European Fascism.

The book represents an overdue revision of former Communist opinions about Fascism. Not even canonized saints like Georgi Dimitrov are spared! His definitive speech about Fascism before the Comintern (1935) is criticized as a "too stiff application" of the term; Dimitrov is said to have applied definitions and generalizations more from the point of view of a "practical struggle of the day" than from that of the historical truth about the cardinal question: "What is Fascism?" To substantiate their points, the authors quote Trotsky, Radek, Salvatorelli, and Gramsci—a surprisingly broad sweep. They also compare Togliatti's and Dimitrov's differing positions in interpreting the fascist phenomenon.

The book recognizes the differences between conservative fascist movements and radical anti-capitalist ones. It also recognizes that on a very low level of development no fascist movement is possible and warns against classifying every military takeover or royal dictatorship as fascist. Thus, the authors do not consider Franco an outright fascist, and they disagree with Bulgarian Communist historians who consider everything that happened in Bulgaria after 1923 as "fascist."

The authors face a problem encountered also in the West. The very moment they abandon the safe dogma of the previously maintained "party line," they have to fight with the elusiveness and difficulty of defining fascist phenomena. They call Fascism a middle-class movement—while recognizing that it was ineffective in countries with old, genuine middle classes. They consider it to have been more effective among the new *déclassé* middle classes and unorganized workers, whose fathers were neither workers nor of middle-class status in the previous generation—possible they were demoralized serfs. They still refer to any fascist quest for social reform in quotation marks, using terms like the fascist "left," "radical," "anticapitalist," or "revolutionary" tendencies. Yet they recognize the dynamism and mass-movement character of several fascist movements. Amidst the alleged the-

oretical origins of Fascism we find them citing a dozen nineteenth-century ideas, from Christian Socialism to Nietzsche. Yet one all-important forerunner, populism, the father of the "völkisch" idea, is omitted.

Almost all European fascist movements are analyzed, even those of the Baltic countries, but the different movements do not get equal time. In Eastern Europe fascist movements are anti-Soviet, in the West they are antiliberal, and not all of them are racist.

Although rightfully elaborating every ruling-class collaboration with fascist takeovers, the authors do not touch upon, and much less do they explain, the role of the leftist or Communist failures in the rise of fascism. Among the "objective" factors contributing to the rise of Fascism they do not mention the role of the Bela Kun regime in the success of the Hungarian counterrevolution and Fascism; nor the consequences of Stalin's "Socialism in one country"; nor the effects of collectivization horrors in predominantly peasant Eastern Europe; neither do they mention the effects of the Soviets' transforming Communist "internationalism" into a strictly one-way street. The rise of Italian Fascism is narrated without the slightest reference to the failures and shortcomings of the Italian Left between 1919 and 1922, although this failure mystified even Lenin, Trotsky, and Sorel. When talking about the factors which made possible the rise of Nazism there is no mention of the fact that Stalin forbade any Communist collaboration with the Socialists, let alone with bourgeois anti-Nazis.

How then do the authors explain the spectacular collapse of the largest Communist Party in Europe outside the USSR, the German Communist Party, within a few months after Hitler took power? Why was it that in the Right which profited from the Depression all over Europe (perhaps with the exception of such remote places as Serbia and Greece)?

The whole Horthy system in Hungary, and that of Pilsudski in Poland, is considered fascist, but not King Carol II's in Romania. In Hungary this would undeservedly lump together Horthy, Kállay, and Bethlen with Gömbös and Imrédy. The Arrow Cross movement is not dealt with. It is simply mentioned as a "Nazi-type totalitarian movement which was anticonstitutional." Considering Szálasi's fanatic insistence on constitutional procedures and Hungarism's ideology this definition does not seem to be an exhaustive one. Neither is there any word about the Romanian Legion's ideology. Nor are the Romanian bourgeois fascist movements mentioned (Goga-Cuzists, Vaida or Manoilescu, or King Carol II's movements).

In their conclusion the authors consider that the primary goal of Fascism is to freeze all social and political movement and to redirect all energies toward national aggrandizement and expansion. This seems to be absolutely false in the case of the Arrow Cross, the Legion, or Hedilla's Falangists, and a number of other fascist movements, like that of Doriot.

The value of the book does not lie in any new revelation about Fascism. It is rather a milestone in the evolution of thought and reinterpretation of fascist phenomena by East Bloc scholars. Nowhere is this process so advanced as in Hungary. This is not bad for a start. The work accomplished during the last ten years by Hungarian scholars in this field makes us hope that the book of Ormos and Incze will not be the end.

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DOREEN COLLINS. *The European Communities: The Social Policy of the First Phase. Volume 2, The European Economic Community, 1958-72.* London: Martin Robertson. 1975. Pp. ix, 286. £6.95.

The second of two volumes concerning the social policy of the European communities, this study focuses on the European Economic Community (Common Market), 1958-72. The first volume (rev. *AHR*, 82 [1977]: 628) described the social policy of the European Coal and Steel Community, 1951-70. Together the two volumes constitute a highly useful discussion of two decades of social policy development within the European Communities.

This second volume is the more substantial, in part because the topics discussed are more varied and complex, and in part because the treatment is more analytical and evaluative. Proceeding from a review of those sections of the Treaty of Rome which relate to social policy, Doreen Collins describes the application of the treaty in the areas of employment, wages, and working conditions, the movement of the working population, and supportive social services. Throughout these chapters, the author clarifies what actually constitutes social policy under the treaty. Noting that the founders of EEC did not formulate specific statements of responsibility for defined social ends, and that economic and social changes since 1958 have brought new issues to the fore (for example, treatment of migrants, collective bargaining at the Community level, and resource redistribution), the author describes those areas of social policy concern for which there has developed a greater understanding if not an articulated and implemented policy.

Part two of the volume, which goes beyond the

descriptive narrative of the earlier chapters, focuses on problems of social harmonization (equality, equivalence, and social security), institutional attitudes, and policy developments in the more recent period, and on the past and future of social policy within the European Communities. This last chapter is an exceptionally well-written and well-conceived analysis of the limitations within which social policy has been articulated and implemented. Collins argues convincingly that a fully developed social policy must assume the existence of a single economic unit possessing appropriate political authority and that experience has indicated the impossibility of pursuing social policy very far without first identifying social goals.

While the author does not appear to have interviewed EEC personnel, the research is otherwise thorough. The tables, bibliography, and notes are useful. The myriad of detail—facts and figures—although perhaps necessary, does not allow for easy comprehension of an admittedly complex and multidimensional topic. The first analysis of its kind of the role of the European Communities in the area of social policy, this work establishes a high standard for those which will follow.

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GĀMINI SALGĀDO. *The Elizabethan Underworld*. Totowa, N.J.: Rowman and Littlefield. 1977. Pp. 221. \$10.00.

Historians have only recently begun to investigate the social dimensions of crime, poverty, vagrancy, and other forms of deviance in Tudor and early Stuart England. The current attempt to penetrate beyond the picturesque anecdotes and rhetoric of contemporary literary sources involves a systematic assault on hitherto-neglected archival evidence, especially that susceptible to quantitative analysis, and the conscious application of conceptual insights borrowed from various other disciplines, notably anthropology and sociology.

This work has already gone some way toward filling the historiographical void noted and deplored by A. V. Judges in the preface to his celebrated anthology of low-life literature, *The Elizabethan Underworld*, first published in 1930. Although reissued in its original format in 1965, the price of this magnificent facsimile must have deterred many potential buyers; so it was both clever and helpful of Gāmini Salgādo to bring out in 1972 an engagingly-entitled paperback anthology, *Cony-Catchers and Bawdy Baskets*, which included among its ten extracts no less than eight of the pamphlets

by Walker, Awdley, Harman, and Greene printed in Judges' compilation.

Salgādo's second venture into the Elizabethan demimonde is less fortunate. This brief, impressionistic survey seeks merely to "convey the colour and energy of the underworld," a realm to which the author rather capriciously assigns alchemists and astrologers, in company with the predictable beggars, gypsies, prostitutes, rogues, thieves, and vagabonds. There is certainly no shortage of "colour" in the extensive quotations and paraphrases from familiar literary sources which constitute the greater part of the text; they are interspersed with some forty-five black and white reproductions of contemporary illustrations (many also printed by Judges). This potpourri is loosely held together by a banal commentary devoid of analytical purpose, presenting what can only be characterized as a tabloid view of Elizabethan society which, while it may titillate the general reader's appetite for the exotic and sensational, makes no demands on his imagination or intelligence.

In short, unlike Judges' book, whose title Salgādo has so brazenly pirated (along with some suspicious echoes of Judges' text: cf. Salgādo on the origins of the gypsies, p. 152, with Judges, pp. xxiv–xxv), this is no work of scholarship, but a piece of low vulgarization. Indeed, it constitutes a mockery of Judges' appeal in 1930 for a "conscientious exploration" of the Elizabethan lower depths, to which Salgādo is evidently as oblivious as he is ignorant of the published work of those scholars (Beier, Langbein, Samaha, Slack, Walter, Wrightson, and others) who have begun to supply the need to which Judges drew attention almost fifty years ago.

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MAURICE ASHLEY. *General Monck*. Totowa, N.J.: Rowman and Littlefield. 1977. Pp. 316. \$15.00.

EDWARD J. COWAN. *Montrose, for Covenant and King*. London: Weidenfeld and Nicolson. 1977. Pp. vii, 326. £8.50.

Probably no other prominent figures of the English Civil War could provide such a contrast as James Graham, Marquess of Montrose, and George Monck. The first was so chivalric and gallant that he became an apt subject for a romance by Sir Walter Scott. The second, originally characterized by Cromwell as a "simple-hearted man," never quite lived down a reputation for being stolid and unimaginative. Yet it was Monck who almost singlehandedly engineered the Restoration with no bloodshed (a most remarkable feat) while Montrose, celebrated for his spectacular deeds in Scot-

land, accomplished nothing lasting and, in some ways, weakened the cause of the Stuarts in his country. Despite their relative achievements, it is Montrose who has been the favorite of historians and Monck who has been virtually neglected.

The standard biography of Montrose is by John Buchan and it is reasonably thorough and elegantly written. Edward J. Cowan does not match his predecessor's literary excellence, but his is the more detailed and scholarly effort. Based almost exclusively on original materials, it has benefited from recent scholarship in Scottish history. All biographies of Montrose provide ample evidence of his military exploits, and the present work is no exception. Cowan, however, concentrates more heavily on Montrose's constitutional formulations, which have received somewhat less attention in other studies.

The subtitle of the book, "For Covenant and King," reveals the author's main thesis. For it is Cowan's belief that his protagonist remained loyal to the original spirit of the national Covenant even though he switched sides and fought for the Royalists. By supporting Charles I, Montrose opposed the designs of the "few," that faction which aimed to destroy the Scottish Constitution. In a statement that some may question, Cowan asserts that Montrose "was perhaps the most scrupulous observer of the time-honored Scottish constitutional tradition" (p. 43). What the author does not explain, however, is why such a moderate stance as Montrose allegedly took failed to win support from any segment of Scottish society. This remained the case even after he won all his victories. It would seem that Montrose's political thought is less important than the fact that contemporaries saw him as an adventurer only too willing to use the hated papists in Presbyterian Scotland. Moreover, by associating Scottish royalism with his own name Montrose probably did the king a disservice, for he made it impossible for others to oppose the covenanters, since they would then be connected to the hated Montrose. One can very easily take a more cynical view of Montrose than this book suggests.

Maurice Ashley, one of the most prolific of English historical writers, is also well disposed to his subject, whom he believes "was cast in a heroic mold." This book is the first substantially researched biography of General Monck and it is long overdue. It provides a detailed account of Monck's many-faceted career, demonstrating the numerous ways in which he served his country well. Undoubtedly his role in bringing about the Restoration is the deed for which Monck will always be remembered, and Ashley narrates this aspect of Monck's life very ably. Receiving the full view of George Monck in a biography enables us to better appreciate both his motivation and the skill-

ful manner in which he brought about Charles II's return. Our knowledge of the process of restoration is greatly enhanced by this book.

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HOWARD NENNER. *By Colour of Law: Legal Culture and Constitutional Politics in England, 1660-1689*. Chicago: University of Chicago Press. 1977. Pp. xx, 251. \$16.00.

In the Quo Warranto action initiated by Charles II's ministers against London's charter in December 1681, the City authorities were alleged to be guilty of "oppression of the Kings Subjects by Colour of Law" (p. 65). In this book Howard Nenner shows how almost all of those involved on every side of the portentous political conflicts that disturbed England from 1660 to 1689 sought to justify their respective positions by giving them that same color because it was one that commanded universal respect.

The announcement on the dustjacket informs the potential reader that Nenner presents an "original and persuasive argument" concerning the political history of the period. The first part of his argument is that G. M. Trevelyan was both "Whiggish and wrong" when he described the Revolution Settlement as "the triumph of the Common Law and lawyers over the King, who had tried to put Prerogative above the law" (p. 62). What it really represented was a victory for the particular interpretation of the law put forward by Parliament's champions over the interpretation of those who championed monarchical absolutism. Having made the same point in a monograph published some seven years ago, I would agree that the first part of the argument is persuasive—but would demur as to its originality. The second part of the argument, that the political issues of the day were usually thought of and expressed in terms of analogues and metaphors drawn from such areas of the private common law as property and contracts, is both original and convincing.

It is true that a majority of the examples Nenner gives of political positions expressed in legal terminology are of statements made by lawyers, and it might be objected that the members of every profession tend to employ the terminology of their profession in debates; but he provides us with enough instances of the use of legal analogies by nonlawyers to make a convincing case. After the traumatic experiences of the Civil War and Interregnum, Englishmen of almost every political stripe in the Restoration period were anxious to avoid taking any positions or actions that were not

in accord with what they believed to be the "fundamental law." When it came to removing James II from the throne and disregarding the established rules of both private and royal inheritance, however, the best that could be managed, Nenner points out, was to impart merely the "colour of law" to the Revolution Settlement.

This is a well-written, interesting, and important book that should be in every university and law school library.

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JOHN R. MILLBURN. *Benjamin Martin: Author, Instrument-maker, and "Country Showman."* (Science in History, number 2.) Leyden: Noordhoff International Publishing. 1976. Pp. xii, 244. f 63.00.

Benjamin Martin's broad range of accomplishments make him an interesting eighteenth-century figure. His personal aim was a "universal acquaintance with nature, as far as it falls within the compass of human understanding. . . ." Martin was a prolific author, who sought to convey the results of his self-education broadly and effectively. He was one of the first of the itinerant lecturer/demonstrators seeking to "spread the gospel of Newtonian Experimental Philosophy" in England. His many publications on optics, optical instruments, horology, navigation, astronomy, and microscopy were written for "British youth of both sexes" and all those who sought to educate themselves. He was important in the popularization of science in the eighteenth century.

John R. Millburn has written a meticulous biography of Martin, based upon thorough research. He has examined Martin's many published works (listed in appendix 1) and a wide array of newspapers, registers, lists, catalogues, and records. This is surely the definitive accounting of Martin's personal life and career as an instrument maker.

Millburn's work is the second in the publisher's Science in History series, intended to "deal with the common ground between the history of science and social and economic history." This book achieves something rather different. It is a history of scientific instruments and their maker. Martin's design, production, improvement, and sale of scientific instruments is the major emphasis of the book. The instruments sold at his Fleet Street shop are enumerated and described. Martin's role in the resupply of scientific equipment to Harvard College after fire destroyed Old Harvard Hall in 1764 is discussed. Millburn succeeds in his own intent of conveying "some appreciation of the technical and commercial pressures under which the eighteenth-century instrument-makers laboured."

Martin's scientific writings are treated lightly. The substance of his lectures, and his work on natural philosophy in general, are considered only briefly. He is labeled a proponent of "Newtonian Experimental Philosophy" without further elaboration. This is a pity, because Martin's works show him to be an interesting kind of "eighteenth century Newtonian." He was a skilled mathematician, as well as instrument-maker, whose attentions ranged widely. His popularizations of Newton's works might have been analyzed for their assumptions as well as for content, and he needs to be placed more fully in context with other Newtonian popularizers, such as Desaguliers and Hauksbee.

Millburn's biography brings Benjamin Martin to the fore; Martin's "Newtonian philosophy" deserves further investigation.

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J. DOUGLAS PORTEOUS. *Canal Ports: The Urban Achievement of the Canal Age.* New York: Academic Press. 1977. Pp. xvii, 249. \$14.65.

ERIC PAWSON. *Transport and Economy: The Turnpike Roads of Eighteenth-Century Britain.* New York: Academic Press. 1977. Pp. xx, 407. \$18.75.

These are two works of historical geography, each focusing on an important transport medium, but there the similarity ends. Eric Pawson has written a "macro" account of the innovation and diffusion of the turnpike trust of the eighteenth century, in which he also considers the relationship between the innovation and economic change. J. Douglas Porteous has written a "micro" account of four tidewater canal ports, wherein he argues that such ports were a distinct type of urban settlement.

Pawson begins with a sensible discussion of the role of transport in economic development. He addresses the important question of whether transport innovations are a stimulus or a response (elimination of a bottleneck) to development. Since at least Adam Smith's time, economists have known that increased transport efficiency expands the market, which thereby fosters economies of scale in production—but is it the "prime mover," responsible for engineering growth? The timing of the innovation is, of course, crucial in answering the question, and Pawson concludes that the emergence and diffusion of the turnpike trust mirrored, rather than preceded, development.

The innovational character of the turnpike trust was not that it fostered road improvements, but that it was a new way of financing and administering such improvements. It was an attempt to

shift the cost of repair and maintenance from the parish in general to the road user. Yet the trusts were local entities, sponsored by local office-holders, landowners, and merchants, and usually run by local people. The trustees were empowered to borrow funds to make road improvements on the security of future toll revenues. According to Pawson, and contrary to the prevailing view, most trusts were successful in these endeavors.

Pawson clearly documents the spatial and temporal diffusion of trusts. Although the first trust was sanctioned by Parliament in 1663, the boom in turnpike building did not come until the mid-1740s, the years Phyllis Deane and W. A. Cole (*British Economic Growth 1688-1959*) have identified as exhibiting a revolutionary increase in the production of a wide range of commodities.

The question remains, how important an innovation was the turnpike trust? Pawson does not argue that the roads alone would have been capable of carrying the increased flows of raw materials and finished products created during the industrial revolution. The system remained an unconsolidated patchwork of variable quality. He does argue that it enhanced those processes (specialization, centralization, expansion) that were to be intensified by the newer transport innovations of the late eighteenth and early nineteenth centuries. Pawson has successfully defended the turnpike system against those who have tended to ignore its contributions (a group which includes J. D. Porteous), without claiming too much for it.

Porteous begins by reciting the familiar but important benefits of the canal system. Canals facilitated the switch to a coal-based economy by providing a more efficient means of carrying that high volume, low value raw material; they offered an outlet for idle investment funds; and their construction provided employment opportunities during a time in which the workforce was increasing dramatically. But this is not the focus of the book: it is, rather, the canal as a generator of new ports and shaper of industrial regions. To Porteous this is a "greater feat" than those listed above. This is hyperbole.

What follows is a discussion of the rather obvious impact of canals on existing and newly created settlements, and detailed case histories of four such ports (to be found at the intersection of canals and navigable rivers): Runcorn, Stourport, Ellesmere, and Goole. The interesting part of the discussion concerns not their original development, but how each port adapted to railways and highways. Those ports successful in capitalizing on their early strategic location by attracting industry (Runcorn, Stourport, and Ellesmere), continued to grow, while becoming less dependent on their port activities. They did this at the expense of their

uniqueness. Goole, which remained essentially a transport-based town, stagnated.

Porteous concludes by offering a canal port "model" in the form of an extended narrative. It is unconvincing because it is descriptive rather than explanatory, and a brief sketch of the history of Grangemouth hardly serves as a satisfactory test of the model. The book provides a good discussion of the adaptations made by four towns to the evolving economic environment.

There is some indication that the publisher meant these books to be complementary. They are not, and of the two, Pawson's will appeal to a wider audience.

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DAVID LEVINE. *Family Formation in an Age of Nascent Capitalism*. (Studies in Social Discontinuity.) New York: Academic Press. 1977. Pp. xiv, 194. \$14.50.

Ever since its emergence as a specialized field of research, historical demography has been remarkably productive of new knowledge. Of all the branches of our discipline, it is the one most adaptable to social-scientific technique. This strength, however, has also proved to be a liability, and, for reasons that are both ideological and organizational, historical demography has recently found itself facing a number of dead ends—often unable to explain what its own research has revealed. Preoccupation with quantification, seduction by structuralist sociology, and a stubborn insistence that biological behavior can be understood in isolation from other aspects of historical change have all contributed to this regrettable situation. Now, with the publication of David Levine's study of English families from the sixteenth through the mid-nineteenth century, a way out has presented itself. By integrating demographic with political and economic history, Levine's important study challenges the conceptual and methodological orthodoxies of the past two decades. As such, it deserves the attention even of those who would otherwise dismiss historical demography as fatally involuted.

The basis of Levine's study is the reconstitution of families from the parish records of four English communities: Shepshed and Bottesford (both in Leicestershire), Terling (Essex), and Colyton (Devon), each representing a different social and economic evolution. Shepshed, the prime exhibit, is a village with a long history of rural industrialization. Its framework-knitters were already fully proletarianized in the eighteenth century, when their trade enjoyed its greatest period of prosper-

ity. Shepshed is contrasted to the agrarian economies of Bottesford, Terling, and (after 1700) Colyton. Each is shown to have its own demographic history, which Levine relates to the specific mode of production, market situation, and political history of the individual community.

The Shepshed population appears to have been adapting itself to the various mutations of capitalism since the sixteenth century. The knitters there broke with peasant and artisan traditions of marriage, fertility, household composition, and sexual relations at an early date. In the boom period of rural industry, Shepshed workers married younger, had more children, and developed relatively flexible relationships between the sexes. When their livelihood collapsed in the 1830s and 1840s, they adjusted again by changing their household composition and fertility levels, while retaining the pattern of early marriage. The other villages, each of which was affected by the capitalization of agriculture, produced different patterns of change. But there too, family strategy was remarkably flexible, responding in complex ways to shifts in material and political conditions.

Confronted with evidence of complex changes well before 1800, the demographers' cherished notion of a dramatic transition marking the turning-point from preindustrial to industrial society crumbles. In Levine's work the industrial revolution becomes a terminal rather than a starting point. The dichotomous concepts of modernization theory—tradition/modernity, rural/urban, preindustrial/industrial—are shown to be inappropriate, indeed misleading.

In this short space it is impossible to do justice to Levine's sophisticated thesis, an argument that challenges the recent tendency to explain intimate behavior exclusively in terms of *mentalité*, but one that never loses sight of the importance of values in determining reproductive behavior. The author avoids idealist and materialist reductionist positions by insisting that demographic history must be situated in the context of changes in society at large. The author's synthetic approach rescues the study of the family from sterile isolation by bringing it together with a Marxist perspective, the mode of analysis that historical demographers have most insistently rejected. There are aspects of the book that both camps may wish to dispute. Family reconstitution in the absence of precise occupational information is, as Levine himself admits, problematic and leaves open questions about the exact fit between economic and demographic change. It must also be noted that his limited selection of communities does not allow the author to develop a theory of change as inclusive as those he is refuting. Despite these qualifications, however, it is clear that Levine has produced a work of

methodological sophistication and bold vision that carries forward the early research of J. D. Chambers and also meets the standards established by the recent work of Michael Anderson. This is one of the rare books that deserves the attention of all historians interested in the social formation of Western society.

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G. W. BOWERSOCK *et al.*, editors. *Edward Gibbon and the Decline and Fall of the Roman Empire*. Cambridge, Mass.: Harvard University Press. 1977. Pp. xii, 257. \$11.00.

To commemorate the bicentenary of the publication of the first volume of *The Decline and Fall of the Roman Empire*, a conference was organized by the American Academy of Arts and Sciences. On January 7, 1976, twenty scholars assembled on the Campidoglio to begin three days of reading and discussing papers. Those papers—but not the discussions—have been presented in the work under review. For our purposes it will be convenient to divide the papers into three categories: 1.) biography; 2.) intellectual history; 3.) historiography.

1.) David Jordan and Martine Watson Brownley indicate at what cost Gibbon triumphed over psychological, social, and physical problems. Gibbon retreated from English society and from much of life and "created 'the historian of the Roman Empire'" (Jordan). One comes closest to the man in John Clive's study of Gibbon's humor, perhaps the gem of the collection; the man who could casually dismiss Voltaire as one who cast a "keen and lively glance over the surface of history" had an independent and coherent view of things.

2.) Frank Manuel traces Gibbon's debt to French thought, a debt which made him a "philosopher" rather than an erudite; he was thus able to organize great masses of material because he had a theory of the "springs" of Rome's expansion, apogee, and decline. Robert Shackleton makes a solid case for Gibbon's great debt to Montesquieu. J. G. A. Pocock develops this analysis in a rich and subtle paper which places Gibbon in the civic humanist tradition; his explications of key passages are models of hermeneutic method. François Furet shows Gibbon's reliance on the sociology of the day, based on a sharp distinction between savagery, barbarism, and civilization.

3.) On the *Decline and Fall* itself and its place in Roman studies less instruction is offered, the exception being an illuminating paper by Owen Chadwick on Gibbon's use of the Church historians. For centuries sacred and profane history were separate, and this distinction had recently

been sanctioned by Voltaire himself in the *Encyclopédie*. Gibbon broke new ground by including ecclesiastical and theological matters as a constituent part of his narrative. Furthermore, he made free use of both Protestant and Catholic authorities, using the information and sources they preserved while deleting doctrinal and dogmatic distortions. Gibbon thus broadened Roman studies immensely, while maintaining an unbiased (or, at least, non-sectarian) position—surely not the least of the Enlightenment's triumphs.

Every reader of this book will understand Gibbon much better and will appreciate both his place in eighteenth-century culture and his own unique character. That is the measure of the debt we owe the American Academy of Arts and Sciences and the distinguished scholars who answered its call. Biography and intellectual history are better served than historiography, but for the latter students can turn to a symposium organized—quite appropriately—at Lausanne: *Gibbon et Rome à la lumière de l'historiographie moderne*, edited by P. Ducrey (1977).

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DOUGLAS G. LONG. *Bentham on Liberty: Jeremy Bentham's Idea of Liberty in Relation to His Utilitarianism*. Buffalo, N.Y.: University of Toronto Press. 1977. Pp. xvi, 294. \$22.50.

In the great liberal tradition that has been England's bequest to the world, the figure of Bentham has stood as an anomaly and perplexity. Even those who have given him a place of honor in that tradition have recognized that he was a liberal with an important difference. Others, finding that difference so large as to deny him any right to that title—except perhaps in the dubious sense of the useful term coined by Jacob Talmon, "totalitarian liberal"—have yet to account for the influence and respect he has enjoyed even in the mainstream of English liberalism.

The author of *Bentham on Liberty*, Douglas G. Long, does not formulate the problem in quite these terms and therefore does not entirely resolve it. But he does provide some of the material which might enable us to think more clearly about it. Concentrating on Bentham's early years (the second major commentator to do so, perhaps in an attempt to simplify the problem), and drawing heavily on the manuscripts at University College, London (the voluminousness of which constitutes another good reason for limiting the scope of research), Long has written the first serious system-

atic study of the idea of liberty which is at the heart of the problem. His analysis draws attention to two concepts which differentiated Bentham's idea of liberty from that of other liberals—utility and science. These determined the structure of Bentham's thought, and insofar as liberty played a part in it, it was as a function of the other two.

When, for example, Bentham advocated the gradual abolition of slavery, his attitude, Long tells us, was "purely utilitarian, without a trace of regard for the intrinsic value of liberty" or of such other traditional liberal values as "dignity, humanity, or rights"; his argument against slavery was based entirely on the "superior capital value and productivity of free labor." In his analysis of law and government Bentham made "security" paramount; it was because he posited (in Long's words) a "symbiotic relationship between individual security and governmental power" that he insisted upon the unlimited nature of sovereignty. In legislation his operative concept was "happiness," which again made for unlimited power, since the legislature was authorized to do whatever was required for the happiness of the citizenry. In all these instances and many more, liberty had at best a "negative" and "marginal" status; it was whatever was left over after government exercised its legitimate power for its legitimate purposes. Moreover, Bentham's "monolithic" conception of authority, focused entirely upon the figure of the sovereign legislator, contributed further to this "impression of authoritarianism."

The same impression, and the same effect, came from Bentham's preoccupation with science, his attempt to create a "science of society based on a science of man." It was the scientist *cum* legislator, carrying out the mandate of utility, that made authority so compelling and intrusive—and liberty, by the same token, so marginal and precarious. In a telling comparison with B. F. Skinner, Long portrays both thinkers as seeking a form and degree of social control that was very nearly absolute. Bentham would have agreed with Skinner that "the question of freedom never arises" because control is exerted not over the "final behavior" of a person or group but over the "inclination" to behave.

From all of this—and from Bentham's own pronouncements ("I would no more use the word liberty in my conversation when I could get another that would answer the purpose, than I would brandy in my diet, if any physician did not order me: both cloud the understanding and inflame the passions")—one might assume that Long associates himself with the school of thought represented most notably by Halévy, who saw in Bentham's utilitarianism a deliberate devaluation of liberty. Yet Long repeatedly, if not consistently or effec-

tively, tries to dissociate himself from this school. Bentham, he insists, "was not, and did not believe his principles to be, opposed either to liberty or to 'rights'"; the "ascendancy of security as a value" in his system was "at the same time the ascendancy of a form of liberty"; although he did not seek to "maximize" liberty, he did want to "secure" it; his idea of liberty, while "negative" rather than "positive," "utilitarian" rather than "libertarian," and "scientific" rather than "romantic," was nonetheless a real and significant form of liberty. But these protestations, in the light of the evidence Long himself presents, are not persuasive.

It is not until late in the book that Long's quarrel with what may be called the Halévy school becomes clearer. He then suggests that where they have attributed Bentham's illiberalism to his "rage for order," he himself attributes it (or something like it) to his enthusiasm for science, for the "perfection of knowledge." But Halévy, and others following him, have hardly been unaware of this aspect of Bentham's thought. Michael Oakshott put the case most strongly in an article published in the first issue of *Scrutiny* almost half a century ago, when he made Bentham the very model of the philosophe, the rationalist with an overweening confidence in science and an ardent desire to apply science to the entire range of human and social affairs.

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ROGER ANSTEY and P. E. H. HAIR. *Liverpool, the African Slave Trade, and Abolition*. (Occasional Series, number 2.) Liverpool: Historic Society of Lancashire and Cheshire. 1976. Pp. vi, 244. £8.00.

The authors of the essays included in this volume obliquely but effectively engage the fundamental question that concerns most students of the slave trade and British abolition. Was the slave trade as unmitigatedly profitable and exploitative as it appeared at the time of its abolition in the early nineteenth century? They respond with a qualified "no," framed within an appropriate moral revulsion that such a trade should have existed at all.

The papers date from a seminar held in 1974, but most incorporate subsequent revisions and are far from dated three years later. Taking for granted Liverpool's premier rank among British slaving ports in the second half of the eighteenth century, they explore the implications of recent research in Britain on the effects of slaving on the economy of West Africa, the commercial organization of slaving ventures, slave mortality during the Middle Passage, and the origins of the abolitionist move-

ment that culminated in Britain's withdrawal from Atlantic slaving after 1807.

Several authors emphasize the economic and other circumstances within Africa that conditioned European slaving on the high seas. Marion Johnson reviews the possible effects of slaving on the West African economy. She lists imports to Africa, stressing the importance of cloth and beads as against the conventional emphasis on alcohol and guns. She minimizes the economic damage caused by the trade, concluding that "the actual loss in production in West Africa may have been little more than the consumption of those who were removed" (p. 31). Her qualified rejection of the "underdevelopment thesis"—that the slave trade distorted African politics and economies—sets the tone, which Roger Anstey's introductory essay characterizes as "clinical" rather than "passionate," for the essays that follow. B. K. Drake outlines the commercial decisions facing Liverpool venturers in the African trade and stresses the slavers' sensitivity to varying patterns of rainfall in Africa and to the sophisticated demands of their African trading partners. Herbert S. Klein and Stanley L. Engerman examine differing mortality rates during the Atlantic crossing among slaves from several regions along the African coast. They find that the slaves' ports of origin—and therefore presumably local conditions in Africa—explain more about why slaves died during the Middle Passage than do lengths of voyage or the abolitionists' *bête noire*, "tight packing."

David Richardson uses the unusually extensive commercial records of slave-trader William Davenport to suggest that slaving profits may not have been high (those of Davenport averaged around eight percent in the period between 1757 and 1784), but that their great variability may have lured investors seeking occasional very high returns in a highly speculative enterprise. D. P. Lamb uses data on the tonnage and volume of Liverpool slave ships between 1772 and 1807 to show that vessels often carried fewer slaves than permitted by British regulations. This conclusion implicitly challenges allegations that the law of 1788 (Dolben's Act) sharply reduced the profitability of the trade by placing new and tighter restrictions on the number of slaves carried per ton of capacity. W. E. Minchinton, who directed some of the research underlying the essays of Drake and Lamb, adds a complementary survey of Bristol's slave trade to Virginia and South Carolina during its early to mid-eighteenth-century peak. Taken together, these essays judiciously explore new evidence to refute the more extravagant abolitionist charges of exorbitant profits and ill-considered cruelty in the slave trade.

The abolitionists receive similarly dispassionate

treatment in the book's three concluding essays. Roger Anstey provides a useful introduction to the historiography of British abolition. He traces the growing sophistication of this active field from the first studies, which reflected the abolitionists' own stress on their moral uprightness, through recent efforts to weave philosophical currents, politics in Britain, economic conditions, and class interests into a comprehensive explanation of this watershed in British imperial history. Seymour Drescher illustrates the complexity and subtlety of modern abolitionist historiography by arguing, contrary to accepted opinion, that the West Indian sugar colonies remained profitable long after the triumph of abolitionism in Britain. He explains abolitionism as the first phase of a dramatic broadening of English concern for the world outside their island, the same impulse that would eventually justify the rampant imperialism of the later nineteenth century. F. E. Sanderson provides brief biographical sketches of Liverpool abolitionists and explains how crusaders against the slave trade could survive in the traders' home port. Their involvement with antislavery at the national level hardly affected their local connections and political fortunes.

All these essays, whether preliminary analyses of newly discovered records or reflective syntheses, individually merit the careful attention of specialists. They collectively create an effect that will force neoabolitionist historians back to the data to defend theses stressing straightforward economic determinism or exploitation.

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RALPH COLP, JR. *To Be an Invalid: The Illness of Charles Darwin*. Chicago: University of Chicago Press, 1977. Pp. xiii, 285. \$15.00.

To Be an Invalid traces the history and treatment of Charles Darwin's mysterious illness and explores its meaning in relation to the development of his evolutionary theory. The book is a thorough and sober piece of research on a somewhat narrow area of medical history, and yet it tells us much about nineteenth-century fashions in diagnosis and treatment. Although there are a vast number of studies of Darwin's illness, none is as meticulous as the present one. In the absence of laboratory examinations much medical history must remain speculative. Bearing this limitation in mind Ralph Colp, Jr. sifts the available evidence to maximum effect.

The earliest reference to Darwin's illness is to be found in a letter written during September 1837, some nine months after the Beagle voyage. This was the period during which Darwin started writ-

ing up his Beagle diaries. The illness was exacerbated during the period between 1839 and 1842, and on July 1, 1849 Darwin began to keep a health diary in which he recorded the nature of his symptoms for the next six years. His most frequently recorded symptom was flatulence; Colp writes that "he lived under the tyranny of gastric flatulence day and night." Darwin's next most common complaints were boils and vomiting. In fact, his health was a source of constant misery to him, so much so that in one letter he exclaims, "Oh health, health, you are my daily and nightly bugbear and stop all enjoyment in life."

All of Darwin's doctors were puzzled by his symptoms, and an array of diagnoses were made, ranging from "catarrhal dyspepsia" to "suppressed gout." Matching this profusion of diagnoses was a succession of ineffective treatments ranging from ice treatment to hydrotherapy. Colp attributes Darwin's "perpetually half-knocked up condition" to the conflicts which Darwin experienced as a result of realizing the religious implications and social consequences of his evolutionary theory. And, indeed, this theory fits the chronological development of his illness and its abatement. After 1872 Darwin stopped writing about controversial problems and his health improved at the same time. In fact, his periods of greatest physical distress coincided with his periods of greatest intellectual activity. This relationship is carefully documented by Colp, thereby invalidating earlier rival theories.

The earlier theories of the causation of Darwin's illness are reviewed, including the suggested causative role of seasickness suffered on the Beagle voyage, "neurasthenia," the eye-strain theory, and the depression theory. Psychoanalytic explanations which focus on Darwin's relation to his father abound, and John Bowlby, not surprisingly, attributed Darwin's illness to a bereavement reaction to the illness and death of his mother. A parasitologist diagnosed Chagas' disease, which Darwin presumably contracted from a black beetle while in South America. This diagnosis, however, does not explain Darwin's freedom from symptoms until almost a year after his return from the Beagle voyage. Other theories examined and dismissed include the possibility of arsenic poisoning and a severe allergy to pigeons. One striking fact which emerges from Colp's study is how much medical diagnoses depend on the perspective and specialty of the practitioner; this is particularly true when medicine deals with historical rather than present evidence. Colp demonstrates how much can be done in this area by a careful and comprehensive investigation.

VIDEA SKULTANS
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SUSAN BUDD. *Varieties of Unbelief: Atheists and Agnostics in English Society, 1850-1960*. New York: Holmes and Meier Publishers. 1977. Pp. vii, 307. \$20.00.

When a book appears bearing the same title and treating a very similar topic as an earlier work, comparison is demanded. Martin Marty's *Varieties of Unbelief* (1964) skillfully explored the decline of religious faith and examined the origins and shape of the modern secularized consciousness. The book was a balanced synthesis, provoked serious thinking, and remains current. Susan Budd's *Varieties of Unbelief* is more appropriately defined by its subtitle, "Atheists and Agnostics in English Society, 1850-1960." Actually, her subject is not even that extensive but is confined to an analysis of the formal associations of such people—the Secularists, Rationalists, Ethicists, and Humanists.

Quite correctly this volume places the beginning of these various societies in the eighteenth-century rationalism expressed by Tom Paine, political radicalism, Owenism, and the anticlerical attitudes of the British working classes of the early and mid-nineteenth century. The activities of and the differences between Secularist founders George Jacob Holyoake and Charles Bradlaugh are well defined. Relying chiefly on primary literature, Budd explores the various manifestations of anti-Christian sentiment in later Victorian times. She devotes several chapters to the Rationalist Press Association founded in 1899 by those who wished to circulate the works of Huxley, Spencer, and other advocates of science as a substitute for traditional religion. Never very numerous, Secularists and related agencies like the neo-Malthusians did play an important role in Victorian social history. In spite of their unorthodox persuasions and their hostility to Christianity, these people, like other Victorians, sought to develop sanctions for social morality and to achieve respectability for themselves. For a more thorough history of the early years of British Secularism, readers would be advised to consult Edward Royle, *Victorian Infidels* (1974).

The second portion of this study takes up the story of the twentieth-century heirs of the Secularists. There is less coherence here, but not through any fault of the author. No longer powerful either intellectually or socially, the English churches now provoke no clear opposition. A twentieth-century Bradlaugh or Holyoake is unnecessary. More important, the belief in progress through reason and the confidence in the beneficence of science which provided the foundations for Victorian secularism have been severely shaken by the experience of the twentieth century. Humanist associations or ethical churches here and there come into existence

but serve only those who still require a structure and ritual for their secularized consciousness.

Susan Budd calls attention to those small groups which have witnessed loudly to the fact that English society has gone through the same process of secularization that all modern industrialized people have experienced. Studies of these often ephemeral groups can help to define the specific nature of that process, and therefore this book deserves the attention of the specialist in modern British history. The more generally interested scholar would be advised to consult either Owen Chadwick's *The Secularization of the European Mind in the Nineteenth Century* (1975) or Martin Marty's *Varieties of Unbelief*.

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MARTHA VICINUS, editor. *A Widening Sphere: Changing Roles of Victorian Women*. Bloomington: Indiana University Press. 1977. Pp. xix, 326. \$15.95.

The emphasis of this fine collection of essays on Victorian women—a sequel to *Suffer and Be Still* (1971), and, like the earlier volume, edited by Martha Vicinus—is on opportunity rather than oppression. Four straightforward historical essays examine the shifts in women's legal, educational, and professional status after 1860. In a study of the reforms of the Married Women's Property Law, 1857-82, Lee Holcombe explains that reform extended the provisions and remedies long available to rich women to women of all classes. A. James Hammerton uses the records of the Female Middle Class Emigration Society, an organization of slight achievement (in twenty-four years it helped only 302 women to emigrate) but intense correspondence, to investigate feminism and female emigration. Rita McWilliams-Tulberg outlines the controversial admission of women to degree status at Cambridge University. Christopher Kent traces the evolution of the profession of actress from its disreputable but lucrative phase at the beginning of the century, when a musical comedienne might make forty pounds a night, to its rise in influence, respectability, and feminist activism.

In a striking study of Victorian death rates, Sheila Johansson argues that the increased longevity of English women was the result of a decline in tuberculosis, and not, as has generally been thought, a reflection of improvements in childbirth and maternity care.

The gem of this collection, however, Judith Walkowitz's "The Making of an Outcast Group: Prostitutes and Working Women in Nineteenth-Century Plymouth and Southampton," deals

with a group of women who were still oppressed, and whose sphere may well have been shrinking. In its patient and imaginative research, its subtlety of reasoning, and its consistent respect for the experience of the individual women behind the statistics, Walkowitz's essay is a model of what women's history should be. Working from local newspapers, hospital and police records, directories, census data, and statements of prostitutes, authorities, and repeal workers, she has reconstructed the backgrounds and the lives of prostitutes in two garrison towns under the Contagious Diseases Acts. Her discoveries about their career patterns, their clientele, and the erosion of their solidarity with the working-class communities in which they lived, after the Acts forced them to make their private lives public, are all important to our understanding of social and community networks, and the problems of intervention and deviance.

The essays dealing with sexuality and sexual identity are less successful. Carol Christ's "Victorian Masculinity and the Angel in the House" does begin to explore the important subject of the ideals of manliness which accompanied the sinister sentimentality of the feminine ideal. But these ideals go well beyond literature, and Christ's essay, drawing primarily on the poetry of Coventry Patmore and Tennyson, seems thin. F. Barry Smith, in "Sexuality in Britain, 1800-1900: Some Suggested Revisions," in fact makes many more suggestions than revisions, and the negative stereotypes of Victorian sexuality against which he argues are already skeptically regarded. Smith's data comes from Carlile and Drysdale, from advertising and advice handbooks—sources as open to criticism as those he challenges. As Smith himself notes, "There is precise and extensive information in census returns, reports on inquests and divorce actions, mothercraft books, and medical journals that remains largely untapped." Some judicious tapping of his own might have made this a more substantial and more up-to-date piece. Sally Mitchell's more historically-aware analysis of the fiction and the advice columns in the lower-middle-class *Family Herald* and *London Journal* provides much fresh information and insight regarding masculinity, femininity, and sexuality.

Finally, all scholars of Victorian women will want to have Barbara Kanner's superb bibliographical essay, "The Women of England in a Century of Social Change, 1815-1914." Part two, in this volume, continues the finely annotated thematic listings from *Suffer and Be Still*, here covering such major topics as marriage and family; women's health and health care; science, social science, and their social application; eugenics; the

beginnings of psychoanalysis; and women's organizations. This extensive bibliography of primary source material, and the texts and notes of the other essays in this book, will widen the sphere of many a weary researcher.

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ROYDEN HARRISON *et al.*, compilers. *The Warwick Guide to British Labour Periodicals, 1790-1970: A Check List*. Atlantic Highlands, N.J.: Humanities Press. 1977. Pp. xxiii, 685. \$45.00.

Labor history is a comparative newcomer to the academic scene, and until the Centre for the Study of Social History at the University of Warwick undertook the monumental task of locating labor periodicals and listing them, the student's task was often fraught with frustration and confusion.

The source material of labor history is frequently found in obscure periodicals which, by their nature, have rarely found their way into libraries. Only in recent years have the majority of librarians taken the collection and preservation of such material seriously. The *Warwick Guide* provides the first provisional check list both to serve as a research tool and, hopefully, to lay a basis on which future definitive editions can be compiled.

The present volume covers over four thousand entries published during the years of industrial development from 1790, and the principles which guided the compilers are set out in the introduction. Periodicals are those journals that were intended to appear at regular intervals for an indefinite period. This excludes such ephemera as strike bulletins or election addresses. But it includes publications which only achieved one issue, whether printed or duplicated.

Some of the problems encountered in deciding what to list and what to leave out are discussed in the introduction. One of the most complex is the chameleon-like nature of many periodicals which first responded to a particular phase of development in the labor movement and then remained in publication to serve different purposes. The secularist press is quoted as an example. In its early days, it played a significant part in shaping working-class consciousness, but toward the end of the nineteenth century was frequently voicing anti-labor-movement opinions.

The compilers stress that the *Guide* is only a check-list and not a definitive bibliography. This will be obvious to American students because holdings in the United States were not included.

To complete the work, a future edition will have to include valuable and extensive holdings such as the Seligman collection at Columbia University, which contains at least one important periodical apparently not available in any library in the United Kingdom, *The Factory Operative Guide and Labourer's Advocate*, edited by J. B. Horsfall of Royton in Lancashire during 1853 and 1854.

In spite of the reiterated assertion that the present volume is merely a base on which to build, for students working in the field it is an invaluable aid to research, and the three compilers are to be congratulated and thanked for their painstaking pioneering efforts.

EDMUND and RUTH FROW
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STANDISH MEACHAM. *A Life Apart: The English Working Class, 1890-1914*. Cambridge, Mass.: Harvard University Press. 1977. Pp. 272. \$12.50.

Standish Meacham's working class was culturally re-created in the years between 1890 and 1914. A greater degree of class unity was brought about by the same economic, social, and political changes that drove bewildered working men and women to seek reassurance and security in a life-style based on custom, tradition, and class introspection. He explicitly follows the important hypothesis presented by Gareth Stedman Jones ("Working-class Culture and Working-class Politics in London, 1870-1900: Notes on the Remaking of a Working Class," *Journal of Social History*, 7 [1974]), but where Stedman Jones' arguments about a retreat from political consciousness sought to explain the behavior of working people in the East End of London, Meacham believes that they apply to the English working class as a whole.

This book is based on the evidence of personal experiences, with the richness that such material will yield, but also the analytical weaknesses to which it can lead. The work is distinguished by this dependence on personal testimony: memoirs and autobiography, evidence presented to or offered by those who carried out contemporary surveys, the oral history interviews in the "Family Life and Work before 1918" archives at the University of Essex, and novels of the period. The years under review are particularly rich in such material, and Meacham synthesizes it well. The outcome is a relaxed, readable, and essentially descriptive book. The everyday aspects of people's lives emerge through his evocative material—the organization of the home, the way wives in particular coped with financial difficulties, the provision

of food, leisure activities, types of work and workplaces. Yet, as one realizes that Meacham is not taking us very far beyond such matters, his concern with the surface of daily routine becomes frustrating. These matters are essential to a study of the English working class in these years, but ought they to contribute the overwhelming part of such a study?

The problem derives from the fact that Meacham is seeking to do more than describe; he wants to explain the way in which working-class culture shifted to cope with the pressures of change. He cites with approval E. P. Thompson's strictures about class as a relationship not a category, yet there is little evidence of this perception in Meacham's presentation. His working class is inert and supine, reacting to change in a bewildered fashion. The book is a description, not an explanation, or even a location, of a way of life; therein lies its analytical weakness. We see this in the description of schooling. Many of these people grew up in the first period of mass education, and Meacham rightly dwells on the experience of school. Its quality and discipline, parental attitudes, and so on all emerge vividly from his sources, but these sources do not lead him to ask how the rise of mass education altered the working-class culture that is the subject of his book. As a result, the question remains both unasked and unanswered. The analytical problem that Meacham posed at the outset of the book is lost in the experiences and memories of individuals, and those emphases implicit in the sources have taken over.

If this absorbing book had not assumed such an ambitious task for itself, it would have been easier not to be so critical. Much of the explanation is simply derived from uneasily introduced quotations from later sociologists. Other explanations are low-level and eclectic, as when "respectability" is repeatedly used to explain particular behavior without examining the meaning or relevance of the term or its position within a wider ideology. It is this lack of attention to ideology (apart from a rather traditional closing chapter) that is the most unsatisfactory aspect of the book, for Meacham's culture is about the surface, everyday aspects of life, rarely touching the ideology or the more fundamental understandings which underpinned that culture.

Meacham makes excellent use of personal testimony, and draws it together into an evocative and rich book which will be much thumbed for its detail. In the end, however, it reveals the limitations of history whose data and concepts depend too heavily on such source material.

GEOFFREY CROSSICK
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RAPHAEL SAMUEL, editor. *Miners, Quarrymen and Slateworkers*. (History Workshop Series.) Boston: Routledge and Kegan Paul. 1977. Pp. xvi, 363. \$20.00.

This book is the second in a projected twelve-volume series on British working-class history. If and when it is completed, the series will make public the fruits of more than ten years of painstaking collaborative effort undertaken by members of the History Workshop at Ruskin College, Oxford, under the general inspiration and guidance of Raphael Samuel. Grouped loosely around the theme of "Family, Work, and Home," five of the books are to be on work, two on childhood, two on education, two on popular culture, plus one other. In turn these works are part of a larger effort, of which the admirable new journal *History Workshop* is another element, through which the Ruskin group is seeking to challenge the specialization process which has removed so much of academic history from all but a professional audience and "to foster a 'peoples' history,' which will bring the boundaries of history closer to those of peoples' lives" (p. xvi).

In this second volume, as in the first on *Village Life and Labour* (1975), Samuel and his collaborators have succeeded brilliantly. This is despite formidable odds, among which not the least has been, predictably, lack of official Oxford University encouragement. Indeed, if the further publications in this series maintain the breadth, originality, and vividness of the first two, then the editor and his fellow researchers will have to be congratulated on achieving what so many younger labor historians, on this side of the Atlantic at least, have paid lip-service to, but have so rarely produced: namely, a rich, multifaceted form of working-class history which provides a convincing and sympathetic portrait of how common folk in a wide variety of occupations actually lived, while at the same time maintaining high standards of traditional historical scholarship.

In this book Samuel has successfully brought together Merfyn Jones, the grandson of a Welsh slate quarryman, Brian Didsbury, a former Cheshire chemical worker, and Dave Douglas, an activist Durham coal-miner, to write movingly and knowledgeably about the way of life of their industrial forebears, without lapsing into either hero worship or vapid anticapitalist rhetoric. The one possible exception to this is Dave Douglas' essay on the Durham pitmen, which seeks to join personal reminiscence and formal historical narrative in what is at times an uncomfortable mixture. At the beginning of his essay, for example, Douglas jumps suddenly from an entertaining description of the colorful clothes worn by Durham coal-min-

ers in the early nineteenth century, to the disapproval registered by older Methodist miners of the variegated clothes of the younger mining generation today. ("They divand look like pitmen these days, mair like pansies" [p. 212].) He does this without putting the significance of Methodism into its proper historical perspective. But Douglas more than makes up for this shortcoming with a detailed and scholarly portrait of the organization of work in the pits, and of the traditions of independence and mutual support which lie behind the coal-miners' long history of trade-union solidarity. For good measure, in the last chapter of the book Douglas also provides us with a glossary of pit talk in County Durham which helps us understand the rich Geordie dialect with which his narrative is larded and without which it would lose most of its cultural authenticity.

Samuel himself opens the collection with a wide-ranging and exceedingly well-informed economic history of the great range of mineral workers—jet makers, limestone workers, sand diggers, copper, lead, and tin miners—whose apparent obscurity has so far led social historians to ignore them. This chapter serves both to set the stage for what follows, and to remind us of how dangerous it is to assume that what we know of underground coal-mining can be applied indiscriminately to the many other kinds of mineral work. Until at least 1875 the preponderance of diggers were not deep-seam coal-miners using modern machines, but pick-and-shovel men coaxing a wide range of ores and metal from shallow indentations in the land. As an instance, up to 1900 the Penrhyn slate quarry near Bethesda in North Wales employed some 2,800 men in what was then the largest mineral-extracting establishment in the world.

It is this Penrhyn quarry, followed by the analysis of the "lumpmen's" and "watermen's" (bargees' or rivermen's) skills and way of life in the Cheshire salt industry, which provides the material for the chapters written by Merfyn Jones and Brian Didsbury, respectively. Each is excellent, providing us with detailed, comprehensive portraits both of the productive process in the trades—the salt industry being dominated by small, family proprietors whose lives revolved around the wych-houses with their boiling salt pans; and the slate splitters of Penrhyn developing such pride of craft that they competed in local eisteddfods—and of their economic structure and development over time. The connecting link between the salt workers and the slate men, and the Durham pitmen too, lay in the autonomy and independence of their organization of work. In both salt and quarrying, the employers tried (and eventually succeeded) to impose modern employment and trading patterns upon the workers, but

not without enormous resistance. In slate-making, a monthly "bargain" was struck between the quarry owners and crew of rockmen and splitters, with payment shared between each crew according to its own fashion. In salt-making, the proprietors owned the pans and supplied the brine and coal, but left it up to the "lumpmen" to decide how much and what type of salt to produce, and at what price. This continued in both trades until at least 1890, by which time virtually all of the British manufacturing economy is usually supposed to have been industrialized.

This book really is grass roots history, not a pretense at it. Trade unionism is cut down to size. " 'Well, bugger me,' exclaimed one Geordie miner, stamping angrily out of his pit upon finding that his union secretary had not complained about the excessive amount of water in which he and his mates had to work, 'are we gaana stand for this lot?' 'Naar,' came the reply" (p. 227). This vignette, tiny though it is, tells us more about how strikes actually start than twenty volumes of official trade-union history. Let us look forward to more of them in the succeeding volumes.

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ROBERT COLLS. *The Collier's Rant: Song and Culture in the Industrial Village*. Totowa, N.J.: Rowman and Littlefield. 1977. \$15.00.

Over the past fifteen years, social historians of nineteenth-century Britain have made use of a large variety of sources previously left untouched or considered suspect. They have rarely, however, made a sustained effort to explore the cultural evidence of history. Although the great writers of nineteenth-century fiction are often included in social history courses, the music, songs, and literature of the working class remains neglected. *The Collier's Rant* is an effort to redress this state of affairs. Limited to the coalfields of the northeast, Robert Colls' study has wider implications about the use of literary, and particularly popular and folk, sources by historians.

By and large Colls has avoided the temptation to romanticize either the men of the "coaly Tyne" or their songs. Nor has he denied the importance of the large number of songs celebrating the apolitical, comic, hard-drinking collier. Some historians, seeking only evidence of militant trade unionism, have derogated these songs as non-working-class, without taking into consideration their enormous popularity. Colls, however, correctly emphasizes the durability of such mythic figures as "Bob Cranky," who thumbed his nose at

all authority. He also makes convincing use of trade-union literature to show the forging of a new political consciousness. His final chapter is a sustained and effective attack on the condescension and arrogance of left-wing social realists of the 1930s, who thought that the most graphic descriptions of filth were the most realistic. Indeed, Colls is at his best when on the attack against the false assumptions of both historians and writers about a culture of which they have little personal knowledge.

Unfortunately, however, Colls appears to have decided upon an almost militantly amateurish approach to his material, so that many of his good points are marred by excessive simplicity or a tendency to assign motives not evident in his sources. Insisting that he has written "an essay" rather than history, Colls provides little continuous narrative, and his research into the wider issues his study raises is very limited. Thus, his discussion of the rise of Methodism and its impact upon pit life—and trade union organizing—is almost entirely derived from E. P. Thompson. Once again Methodism is lashed for its vile effect upon the colorful life of the miners. The understanding and sympathy extended to the drunken braggart is withdrawn from the sober Methodist. Why Methodism should be so appealing to a powerful minority—including many leaders of trade union locals and pit villages—is never discussed. Pitmen and their families responded to the call for greater sobriety, self-discipline, and godliness because it meant greater self-respect and control over their own lives. Whatever we may feel about the cultural and political results, Methodism deserves a more sympathetic hearing than Colls provides.

The Collier's Rant is successful as an introduction to the pit culture of Northeast England, but it will not serve the larger purpose of demonstrating the importance of cultural evidence for the study of social history. Colls' use of his sources is too idiosyncratic and incomplete for that. Yet the richness and diversity of pit life can be found in Colls' book, and hopefully this will spur others on to re-examine not only mining culture, but also the literature of other industries and regions.

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LUCY MIDDLETON, editor. *Women in the Labour Movement: The British Experience*. Foreword by JAMES CALLAGHAN. Totowa, N.J.: Rowman and Littlefield. 1977. Pp. 221. \$13.50.

The absence of close studies of women's roles and contributions to British labor, trade-union, and

cooperative movements over the past century leaves a serious gap in the history of an industrial society in which, by 1900, the number of working women over fifteen years of age was approximately four million (well over one-third of the total female population) and the number of Labour-identified women leaders and women's organizations increased dramatically. Thus, Lucy Middleton's anthology of nine short essays, a biographical index of fifty-seven women leaders, a bibliography, and seven appendixes of statistics on women in the labor movement represents a step toward filling the inexplicable void.

The authors, all women activists in the fields about which they write, render detailed descriptive surveys containing a "who's who" of female participation for over seventy-five years in Labour politics, Labour social services, trade unions, and Labour-supported cooperative organizations. Although none of the chapters is a thorough or analytical study, important historical questions are at least suggested, so that this book is useful as a point of departure for future investigations on the same topics.

For example, Middleton only partially addresses Henry Pelling's assertion that the "New Woman" was almost as important an element in forming the leadership of the Independent Labour Party as the "New Unionism" (p. 22). The same may be said for Sheila Ferguson's description of how the Women's Labour League undertook not only to fulfill the goals of the Labour Party, but also to start programs that reflected traditional female roles in pursuit of a wide range of welfare causes and social reforms. The concurrence of change with continuity in women's activities is illustrated in the succeeding chapter by Margherita Rendel which provides perspective on more radical tendencies within the W.L.L. by highlighting the roles that working-class women members played in the Women's Suffrage Movement (a subject surely worthy of a book-length study). Following is Mary Walker's discussion of the League's work in establishing international socialist contacts. More familiar are the details of women's involvement in trade-union and consumer cooperative organization and administration. But in their chapters on these subjects Anne Godwin and Jean Gaffin leave the impression that knowledge and understanding should now be sought through deeper investigation into the same questions.

In a separate section devoted to "The Movement Today," Oonagh MacDonald, Margaret McCarthy, and Maeve Denby assess the status of women in the Labour Party, in trade unions, in Parliament and government in the 1970s. They conclude that women have been gaining strength and status in each entity, but that full equality with men still

lies ahead. As MacDonald states it: "It is the unequal burden of child care, which, more than anything else, causes and maintains the structure of inequality. Without child care provision, the attempts to establish women on an equal footing with men in public life will only have a limited success" (p. 160).

Although not a work of historical scholarship, this anthology should provoke good questions and, one hopes, new inquiries.

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MICHAEL G. FRY, *Lloyd George and Foreign Policy*. Volume 1, *The Education of a Statesman: 1890-1916*. Montreal: McGill-Queen's University Press. 1977. Pp. xiv, 314. \$18.50.

The Lloyd George industry rolls along in booming prosperity. There are new biographies and more to come. The current trend is toward special studies: Lloyd George out of office after 1922, Lloyd George and labor, Lloyd George's wartime diplomacy with America, and now Lloyd George and foreign policy, the first of two volumes. Superficially this approach would appear reasonable. A full-scale biography of so large and elusive a figure as Lloyd George demands frightening commitments. The story of Lloyd George and something in particular offers a way of dividing a huge project into more manageable compartments. Theoretically, when enough pieces of the mosaic have become available we will have the entire picture.

The trouble is that Lloyd George did not run his life to suit the discrete categories of any historian's mind. He never thought, and he did not work, in terms of single issues or problems. The secret of his success was comprehensiveness of view and flexibility in action. A defeat in one area could usually provide an advantage elsewhere. He thought more rapidly than most people and so could make what was in fact brilliant improvisation appear to be inspired planning. Nevertheless, except for the first two years of his prime ministership, when his closest colleagues could not divert him from attention to the war, it is really impossible to define a Lloyd George policy about anything.

So there is an artificial quality about this book. Michael G. Fry delineates an area of state activity and crams Lloyd George into it. But Lloyd George can never be crammed into anything except the whole world of politics, and still worse, foreign policy, at least until 1916, really held little interest for him. To be sure he made the Mansion House speech, which rates a chapter in this book, and he participated in the Cabinet meetings over the bank holiday weekend in August 1914 that finally took

Britain into war with Germany. Typically, Lloyd George made a moral dilemma of his own hesitancy, and allowed it to be solved by the German invasion of Belgium. In fact he tuned his conversion to war to the evolving opinion of the Non-conformist, pacifist, radical constituency within the Liberal Party that acknowledged him as leader. But he knew perfectly well, as did all of the Cabinet who had attended the Committee of Imperial Defense briefings after Agadir, that the Germans would in the end violate the Belgian neutrality. Fry acknowledges the domestic political pressures on his subject, but the limits of the book do not allow an explanation.

Beyond this Fry has to stretch his definitions. Opposition to the Boer War and the struggle over naval estimates become foreign policy. It is like counting references to food in Shakespeare. The result may be interesting, for some even useful, certainly it is arduous, but we have lost the drama and poetry.

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R. S. SAYERS. *The Bank of England, 1891-1944*. In three volumes. New York: Cambridge University Press. 1976. Pp. xxiv, 680; viii, 403. \$95.00.

R. S. Sayers' *Bank of England, 1891-1944* is the product of a seven-year exhaustive examination of the written and the remembered historical record. As one of the two principal economists who wrote the Radcliffe Report, he is exceptionally well qualified to handle the complex task of weaving an insightful tapestry of the Bank's activities. Sayers marshals his evident skills as an economist and as a historian and creates a scholarly tract which will enhance our understanding of the history of the Bank of England as it was seen by the principal economic actors during the chosen period.

If Montague Norman seems to merit the title of "*primus inter pares*" as a central banker as Sayers argues (p. 592) in the reader's eyes, he will find that he has been snared by the web that Sayers weaves to recreate the atmosphere which surrounded the governor's chair during these years. The bank and the world will be seen as they were viewed by the governors, the deputy governors, their colleagues, and other important personalities whose views had influence. The "story as it has unfolded . . . turn[s] on comparatively few rounded figures" (p. xiii). These figures are the dramatis personae which dictated the bank's actions in London and abroad. Montague Norman, governor from 1921 through 1944, and Walter Cunliffe, governor from 1913-18, are part of a cast of

eminent figures which includes Strong, Niemeyer, Balfour, Lloyd George, Churchill, Schacht, Dawes, Macmillan, McKenna, Pigou, and Keynes.

Apparently statistics, apart from certain balance sheets and interpretive ratios or percentages, and complex monetary theory did not clutter up the heads of the bank's men. Neither the money stock nor the absolute level of commodity prices mattered; only interest rates and exchange rates were seen as exerting an influence, and then only on financial capital movements. The application of monetary policy, then, was achieved by an adjustment in the bank rate and the bank-sponsored actions which made this rate adjustment effective—effective in the sense that it made other domestic interest rates on freely traded financial assets move in the same direction. The influence of this rate adjustment was seen as affecting only "financiers and was not to interfere with the wheels of production, distribution and consumption" (p. 45). The bank saw as its main objective the need to sustain the gold exchange value of the pound sterling in the realm of national economic policy, and to achieve this goal by trying to ensure that its actions exerted only a minimal influence on the normal financial needs for accommodation of domestic commerce. Naturally, in this context, the bank sought to achieve its objective by relying on bank-sponsored movements in the prices of financial assets to produce substitutions between domestic and foreign financial assets which would generate the maximum movement of gold and foreign currencies in or out of London.

The efficiency with which the bank could sustain this main objective, as measured by the degree of bank-initiated activity in the domestic financial market or in the foreign exchange or gold markets, depended on the degree of the bank's monopoly power. This was well recognized, and the bank's servants strove to accentuate it. Its domestic financial market involvement was so arranged that its market power was initially focused on a select group of dependent market operators (discount houses, accepting houses, money brokers, and the like). The efficiency of its international financial market activities depended on foreign confidence that the gold value of externally held sterling would be sustained. In this area, too, the bank found that monopolistic and cartel-type arrangements with other central banks were most useful devices for maximizing the effectiveness of its "free" market operations. Naturally, the degree of success of this central bank collusion depended on the similarity between the goals of the members of the cartel and the degree of freedom such institutions had to bring these goals into being in the face of the pressures which could be exerted by each nation's government.

It is in this sense that Norman was the governor par excellence. He clearly recognized the need for central banks to be established with like goals and similar degrees of independence, and he worked hard to achieve this. Along with Benjamin Strong of the New York Federal Reserve Bank, he laid down the guiding principles of a central bank (exclusiveness, balances, cooperation, and autonomy) and indicated clearly their aim: the maintenance of the gold exchange value of their national currency unit. His success depended in large measure on the degree of secrecy, suasion (moral and otherwise), selectivity, and in-group collusion he could achieve. The bank, to sustain this power, "... had always to advise; to advise ... on matters on which it had deliberately cultivated a mystique that at best befuddled discussion and at worst intimidated those who had to take the political responsibility" (p. 387). Norman, besides being a superb diplomat, had the necessary qualities to accentuate the impact of his advice. His "individualist habits, his pathological secretiveness and his unrivalled experience rarely gave (even) his Deputy a chance to know what was going on ..." (p. 648). He virtually mesmerized his political masters and market operators into approving what he and his close confidants recommended. Only a few leading personages like Keynes remained unconvinced by the governor's wisdom; Keynes describes Norman thus: "always absolutely charming; always absolutely wrong" (quoted p. 602).

As Sayers admits, his chosen approach to the subject will not satisfy all criteria. It is "not a monetary history [and], ... it cannot satisfy economists" (p. xii). Its limitations stem from Sayers' preoccupation with delineating a biographical profile of the bank's men and their associates. It is the history of the Bank of England as seen from a particular point of view, a fact which may well account for the absence of any mention in the 1,000-odd pages of text of the important contributions to this subject which have been made by members of the Chicago School. Its weaknesses as a work in economics again arise from the biographical preoccupation. The bank's men were not especially impressed by statistics; qualitative rather than quantitative considerations dominated their views on credit practices and conditions. Sayers makes no attempt to redress this bias. He is concerned with representing the bank officials' view of the facts as they saw them. He does not process the recorded statistics to show in a relative context the amount of bank intervention. He does not use his mastery of the subject matter to comment on the findings of quantitative empirical work of U.K.-based economists on this period. Finally, as a history of the Bank of England, 1891-1944, it is much more a book for the accomplished

scholar rather than for the novice, and it is primarily an account of the Bank of England as seen by bank officials during the 1914-40 period. Despite these limitations or perhaps *because* of them (one must focus one's energies to do one's work well), Sayers' *Bank of England* is a masterpiece. He has made a unique as well as a valuable contribution to the literature.

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ROBERT PAUL SHAY, JR. *British Rearmament in the Thirties: Politics and Profits*. Princeton, N.J.: Princeton University Press. Pp. xiii, 315. \$18.50.

By late 1937, Britain's policy-makers faced a dilemma depicted by an anguished Maurice Hankey: "It is clear that, unless something can be done to reduce our foreign commitments, risks have to be taken somewhere, either in the field of finance or that of defence" (p. 183). The burden of Robert Paul Shay's account of the hesitant mounting of British rearmament late in that decade is that the National Government ran risks in the defense sector rather than the financial. He depicts an uneven struggle between the Treasury and the Fighting Services for dominance in defense spending, which was "rationed" by policies formulated in the Treasury and approved by the cabinet. Shay links such "rationing" to Chamberlain's attempt to avoid war by appeasement of Britain's likely enemies, a connection first developed in Keith Middlemas' *Diplomacy of Illusion*; yet his book traverses entirely different ground, as Shay explores the process whereby Treasury control of defense expenditure was maintained until the events of March 1939 enabled the Fighting Services to assert the primacy of military needs in Britain's rearmament plans.

Thus through nearly the whole of the decade, Britain's defense strategists were frustrated by Treasury canons of fiscal orthodoxy. While Treasury officials contended that finance constituted a fourth arm of defense, in fact they viewed their efforts to tailor defense spending to the cloth of Britain's fiscal resources as the nation's first line of defense: if that line were broken, nothing worth preserving could be held. As a result, fiscal constraints were placed upon the schemes and, to a lesser extent, the pace of British rearmament; Shay reviews both the exercise and the results of Treasury control. Concurrently, Treasury views mandated organizational constraints: here the unquestioned preference for a "market" economy rather than a "command" model produced governmental deference to business sentiments. In consequence, the business and financial communities took repeated advantage of the official policy

of cooperation. For their part, Britain's political leaders were timid in dealing with these latter-day captains of industry, in Shay's view because of the Tory Party's dependency upon the business community. The actions of the Society of British Aircraft Constructors in particular reveal the profit motive in untrammelled operation; Shay's book breaks new ground in portraying the *business* of rearmament.

In some ways, however, *British Rearmament in the Thirties* does not penetrate deeply into the process of decision-making. Within the Treasury, the enigma of the role of the permanent secretary, Warren Fisher, persists. Fisher distinguished himself as a member of the Defence Requirements Committee, a group of leading civil servants who pressed for rearmament on the basis of perceived military needs; yet the department Fisher headed made a mockery of realistic defense planning. Merely to invoke a "decline of his influence over policy" (p. 74) through these years is neither to understand the man nor to explain his role. Other distortions are introduced by Shay's thematic linkage of a "rationed" rearmament to appeasement: Chamberlain's actions appear wholly determined thereby; his near-messianic belief that he could come to terms with the dictators is quite undervalued. Similarly, the organizational deficiencies of defense planning, mired for much of the period in a confused tangle of departmental and ministerial committees, which handicapped the "experts" in the intragovernmental strife with the Treasury mandarins, are undeveloped.

Nonetheless, there remains much that is informative and valuable in this book: the willful myopia of the National Government is demonstrated in a novel fashion; official deference to vested economic interests, intended to protect the existing social and political order, is revealed; the adverse judgments returned by contemporaries against Baldwin, Chamberlain, and cohorts, questioned by revisionist biographers, are reinforced; the reputations of some defense pundits, including Hankey, Duff Cooper, Swinton, and, most surprisingly, Inskip are refurbished. Although Shay's views do not convince in all particulars, he has uncovered an important aspect of Britain's malaise in the thirties: intent upon the defense of the status quo, Treasury officials, the National Government, the Tory Party, and the business community profoundly miscalculated what constituted national security in the face of the fascist menace.

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JANET MINIHAN. *The Nationalization of Culture: The Development of State Subsidies to the Arts in Great Brit-*

ain. New York: New York University Press. 1977. Pp. xii, 276. \$15.00.

Janet Minihan's *The Nationalization of Culture* comes to us at a particularly auspicious moment. A boiling controversy over the future direction of U.S. government subsidy of the arts is being aired not only in the pages of the *New York Times* and the nation's cultural centers, but indeed across the country—in arts councils, museums, symphonies, theaters, dance and opera companies, as well as in community art centers and by individual artists. The proponents and the opponents of what is being called the "politicization" of the arts are having at each other. Passion-arousing phrases like "populism" and "elitism" are being bandied about along with "clout" and "constituency." It is difficult to find the perspective necessary for informed decision-making in regard to cultural policy for the United States. Minihan's work on the development of state subsidies to the arts in Great Britain helps to give that perspective and will be important to those interested in a rational and informed approach to policy-making in the arts in this country.

The author carefully traces the diverse sources of state support of the arts in Great Britain. Her approach is knowingly complex and devastatingly thorough, and her narrative completely readable. She starts by quoting James Northcote, a student of Sir Joshua Reynolds, from a March 1807 issue of the weekly *Artists* and brings us through to Sean Day-Lewis' interview with Lord Goodman, under whose chairmanship the Arts Council of Great Britain achieved world-wide note. Beginning with painting, she details both the private patronage and the political climate of the early nineteenth century. She describes the change in public attitudes toward the role of the state and relates how these changing attitudes affected not only private initiative and public service, but also private patronage and public subsidy of the arts. Minihan relates the Factory Act of 1833 to the state's responsibility to provide a national system of education, and while describing official views on education she draws parallels to official views on cultural subsidy. From the 1830s the book moves forward, covering the Great Exhibition of 1851 and the doctrines of individual initiative and self-help which Samuel Smiles celebrated. In an informative progression it moves on to the latter half of the nineteenth century and to the nonutilitarian concept of art divorced from morality, from social content, and even from human content, and the crisis in state support that this change in attitude produced. As first film and then radio and television become part of the arts world, the author covers the debate and the laws that lead to their sub-

vention. Finally the history of the Arts Council of Great Britain is examined. Lord Goodman is quoted on his hostility to the notion of a Ministry of Culture, and with this we are brought to present concerns and unresolved questions.

This is an important book. It should serve both as a model and an incentive for the writing of a similar work on this country. It should be read by all those concerned with cultural history and by those who are anxious to participate in the formation of cultural policy in the United States.

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JAMES S. DONNELLY, JR. *The Land and the People of Nineteenth-Century Cork: The Rural Economy and the Land Question*. (Studies in Irish History. Second Series, number 9.) Boston: Routledge and Kegan Paul. 1975. Pp. xiv, 440. \$31.25.

This book is the 1975 winner of the American Historical Association's Herbert Baxter Adams prize. The prize is well deserved. James S. Donnelly has written the most comprehensive case study of the modernization of peasant agriculture that this writer has read. He clearly delineates the transformation of pre-famine Irish subsistence agriculture into modern commercial food production so that famines would not recur and British cities could be fed. The transformation he describes is the equivalent of a revolution in land use; therefore, this book deals with an extremely contemporary problem.

Donnelly accurately describes pre-famine subsistence peasant-farming, which included tribute-collecting landlords and middlemen who had little desire to establish a fully commercial system of food production. Landlords and middlemen were content to receive money rent from a peasantry that acquired the necessary cash by selling a pig or a few bushels of grain; and apart from that they allowed the peasantry to live a subsistence existence under the rundale system of land use that they controlled. Rundale enforced equalized subsistence opportunities, on the basis of soil fertility, by fragmenting and scattering very small land units over the whole area of land under communal control. Commercialized food production would have required that landlords share economic and political power with the food producers living on the land. Before the famine this community of interest did not exist, and after the famine landlords failed to achieve it on mutually acceptable terms.

The famine and its aftereffects (1845-51) are correctly perceived as the turning-point in the modernization of Irish food production. The reduced

number of peasants (by death and emigration), combined with government pressure on landlords, landlord self-interest (to insure a continued rent revenue), and government investment in transportation facilities led to restructuring land use and land tenure institutions. This transformation was so strongly established by 1900 that the outlines of the modern Irish economy and social structure were clearly visible.

The major institutional ingredients in the modernization of Irish food production was the mass conversion of peasant occupancy rights into legal leases, followed in the 1880s and 1890s, after Land League agitation, by making the cultivators into resident landowners. These events are the politically visible aspects of modernizing Irish food production. Donnelly expertly uses economic statistics to show the pull of the English urban market and its reciprocal, the production of food for market sale. He also discounts the interpretation that Irish national politicians and their spokesmen (Irish national historians) have put on the land controversy—that the land and political questions were identical. Donnelly makes a very good claim for a different interpretation.

Donnelly claims that the land question only became linked to the national question because of landlord intransigence: landlords trying to collect full rents during poor crop years and during years of low agricultural prices. By the mid-1870s Irish food producers measured their security by their money incomes, not by the adequacy of food produced and consumed within each peasant community. Irish cultivators were not about to lower their standard of welfare, and when landlords would not cooperate to preserve the total amount of the very substantial welfare gains that they had made in the post-famine years, Irish cultivators became receptive to a program to get rid of landlords in the same way that the landlords had gotten rid of the counter productive middlemen in the years immediately before and during the famine. A national policy promised to do this more quickly than a united kingdom policy.

My one criticism of the book is the lack of clear focus on the mechanical changes that gave visibility to the commercial use of agricultural land. Commercializing food production meant consolidating fragmented and scattered land units and then building boundaries around land units that were large enough so that a marketable food surplus would be produced even during the worst crop years. In Ireland the boundaries were usually made by stone fences, but hedges and drainage ditches also served. Fixed boundaries made it possible for a landlord to make one tenant responsible for paying rent on a specific piece of land and prevent the occupier from subdividing it among his

children. Thus, unless a fenced piece of land was available for lease or by inheritance, marriage was discouraged unless the offspring were willing to emigrate to a city or across an ocean. Fencing was also an incentive for the cultivator to increase his labor input because he would now be the sole beneficiary of any extra labor he expended, a situation that was impossible under the pre-famine rundale land use system, and not fully operative in postfamine times as long as landlords claimed a rent that had no necessary relation to income.

This study is clearly superior to the studies of agricultural modernization made by the economists, anthropologists, and sociologists because Donnelly deals with the dimension of time. Economists tend to make time into an invariant so they can discuss agricultural modernization as an exercise in technology, income, and profits, all of which can be quantified, even when these Western concepts of income and profit, as we understand them, are not understood by the peasantry. Anthropologists tend to examine societies as if they were static, in the sense that what they now observe is what has always been; and sociologists tend to look for social change, like income and status and ways of measuring it, without examining the economic and political events that cause these changes to happen. Only cultural geographers, who must closely examine land use and the social changes which follow changes in land use fully understand the dimensions of the problem of agricultural modernization, and Donnelly has made full use of this rich body of Irish literature. For these reasons, *The Land and the People of Nineteenth-Century Cork* should be used as a model for future regional or national studies of the ways in which peasant societies can be transformed to produce an assured food surplus as the necessary foundation for modernization.

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WILLIAM FERGUSON. *Scotland's Relations with England: A Survey to 1707*. Edinburgh: John Doland; distributed by P. D. Meany, Mississauga, Ontario. 1977. Pp. vii, 319. \$24.95.

The recent emergence of organized Scottish nationalism as a significant force in Great Britain has increased both popular and scholarly interest in the background, circumstances, and consequences of the Treaty of Union concluded in 1707. William Ferguson, who is best known as the author of *Scotland: 1689 to the Present* (1968), originally intended to write about Anglo-Scottish relations within the United Kingdom, but he soon felt the lack of a satisfactory account of the interactions

occurring when Scotland and England were independent. This book is an attempt to fill the gap, is preliminary to the author's first concern, and is intended as a contribution to understanding current devolution issues by means of historical analysis and explanation. These varied interests are reflected in the structure of the book, which is by no means a complete or thorough survey of the subject announced in the title.

The author's theme and real subject are revealed by the following declaration, taken in this instance from page 142 but repeated throughout the book: "... the notion of a Grand Design for a United Britain, constantly striven for by the far-seeing on both sides of the Border, is fantasy masquerading as history. The only antidote to this slick determinism is to examine each specific attempt at union and to establish exactly what it involved. When this is done, each such attempt is revealed as a phenomenon dominated by its own immediate and usually far from simple circumstances." Ferguson is especially strident and repetitive in his denunciation of those, such as Smout and Trevor-Roper, who he considers overly fond of theoretical models and insufficiently "factual."

The selective nature of this survey may be observed when one examines the relative space allotted to different periods. Prehistory to 1513 is covered in a "sketch" of 50 pages which summarizes the best recent scholarship. The years 1513-1603 are described in 46 pages derived from similar sources. The 68 pages giving an account of 1603-88 emphasize proposed and actual unions of the two countries and tend to relate all other interactions to those unions. The author's own research and expertise are most manifest in the 111 pages covering the years 1688-1707. This very detailed account of the numerous factors which provided the context for the Union rejects any belief in its inevitability and highlights the widespread and serious opposition to it in Scotland while carefully explaining the reasons for the ineffectiveness of that opposition. This part of the book is extremely valuable and well done.

In spite of its limitations, this volume is the best survey of its subjects from a Scottish perspective since 1901. It is enhanced by excellent critical notes and a stimulating bibliographical essay.

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HUGH D. CLOUT, editor. *Themes in the Historical Geography of France*. New York: Academic Press. 1977. Pp. xxviii, 594. \$35.30.

Themes in the Historical Geography of France is a collection of thirteen essays, seven by the editor, Hugh

D. Clout of the Department of Geography of University College, London, and the other six by three French and three English colleagues. The book is, at least in part, a response to what the editor describes as a sudden drop in historical interest among French geographers which—he points out—is occurring just as historical geography is gaining momentum both in England and on this side of the Atlantic. The purpose of the volume is to survey—neither systematically nor comprehensively—the state of the subject today. It is, to all appearances, a book by historical geographers for historical geographers, but it is one that anglophone students of French history will find richly rewarding in spite of its miscellaneous character.

Students of French history or geography are aware not only of the close connection of the two fields in the French academic tradition but of the role geography has played in the leading historical school founded by Lucien Febvre and Marc Bloch and now known by the name of their journal, the *Annales*. The relationship is, however, often more implicit than explicit and therefore not easy for the beginner—particularly if he is a foreigner—to grasp, since it is often not clear exactly what the geographical dimension is supposed to add to the historical account beyond local color. Thanks to Clout and his collaborators such confusion has been considerably reduced.

The volume opens with an introductory chapter by the editor on “The Practice of Historical Geography in France,” which deftly orients the reader and which, like all succeeding chapters, is followed by an extensive (if not exhaustive) bibliography including articles from obscure or local periodicals. The second chapter, by Pierre Bonnard of the University of Clermont-Ferrand deals with “Peopling and the Origins of Settlement,” beginning in prehistory and coming down to the Middle Ages. To modernists, this may seem remote; but now that the importance of “*la longue durée*” is widely recognized, they should find this introduction welcome.

Chapter three, by the editor, provides a brief but succinct summary of “Early Urban Development” and is followed by a survey of the “Retreat of Rural Settlement” in the later Middle Ages also written by Clout. Chapter five, by Hugh Prince, explores “Regional Contrasts in Agrarian Structure,” describing and analyzing some of the most important attempts to divide France into major regions (those of Roger Dion, Marc Bloch, and Gaston Roupnel, for example).

A highly specialized and brief discussion of the “Reclamation of Coastal Marshland,” again by Clout, is followed by a very useful essay on “Petite Culture, 1750–1850” by André Fel, which provides an excellent introduction to peasant life. Chapter

eight, by Keith Sutton, treats the “Reclamation of Wasteland during the Eighteenth and Nineteenth Centuries” as a base for the agricultural history of the country and leads directly to the more focused study of the “Historical Geography of Western France” by Pierre Flatrès and the fascinating but unduly long discussion of “Vernacular Architecture and the Peasant House” by Gwyn I. Meirion-Jones.

The final three chapters, all by Clout, form a coherent review of French agriculture, industry, and urban growth in the eighteenth and nineteenth centuries that will be of particular interest to historians of modern France. Even though much of the information organized here is well enough known, it is not as succinctly and conveniently presented in any other single work. Altogether these thirteen essays with their wide-ranging bibliographies and many maps and tables comprise an indispensable introduction for beginners and an invaluable reference work for teachers and scholars of French history. Clout and his collaborators are to be congratulated and thanked.

EDWARD W. FOX
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GEORGE HUPPERT. *Les Bourgeois Gentilshommes: An Essay on the Definition of Elites in Renaissance France*. Chicago: University of Chicago Press, 1977. Pp. xii, 237. \$18.50.

In sixteenth-century France the most dynamic and vital social class consisted of people who were neither bourgeois nor noble, but in between. Highly educated and very wealthy, they held some of the highest offices in Church and state: bishops, presidents of the parlements, treasurers, royal secretaries. While seeking to bury their bourgeois origins, they retained the bourgeois virtues of sobriety and thrift. While claiming the legal perquisites of nobility, they despised the *nobles de race* as uneducated, improvident, quarreling wastrels. This social class is anatomized by George Huppert in his stimulating and well-written essay.

Huppert seeks to chronicle the rise and decline of the French gentry (as he calls them) and to describe their mentality. To accomplish the latter task, the author tries to “get inside the skin” of his heroes. The result is a picture of French society as seen by the gentry, a picture which is interesting and novel, but which leads to a stereotypical vision of various ideal types. All nobles are viewed as swaggering braggarts, constantly in debt, and very much threatened by the gentry. All gentry are viewed as sociable and intellectual; they are astute office-holders, gifted managers of their estates, lovers of learning and virtue. All bourgeois are

seen as industrious, moderately intelligent beings, who somehow lacked the sensibility and the drive to become gentry themselves.

The tale of the rise and fall of the gentry which Huppert recounts is likewise fraught with difficulties. The dates of the story are hard to discern. Many of the gentry's attitudes and practices date from the fifteenth century and continue into the seventeenth. Yet the era when they attempt to set themselves off as a social class, to rule France, and to educate their countrymen in the ways of virtue, is the sixteenth century. Unfortunately, says Huppert, they fail, and attract the scorn of the bourgeoisie, and the hatred of the nobility. Bitter and disillusioned, the gentry follow the advice of their spokesman, Montaigne, and retire to their country estates, fleeing cares and "servitudes." Away from the court, they can meditate on the classics and retain their independence.

The conclusion of the gentry's story fails to convince. Did large numbers of them really give up the fame and offices their ancestors had worked so hard to secure? Huppert writes persuasively, but in describing the alienation of the gentry, as elsewhere in the book, he should do more than recount case histories. Figures are needed as much as examples. Nevertheless, the essay, itself a gentry invention, indicates an "attempt" and does not purport to be a finished product. In this provocative book, George Huppert has given early modern French scholars a wealth of ideas to ponder.

PETER M. ASCOLI
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YVES JEANCLOS. *Les projets de réforme judiciaire de Raoul Spifame au XVI^e siècle*. Geneva: Librairie Droz. 1977. Pp. 122.

Historians of French legal history should welcome this lucid exposition of the ideas of judicial reform presented in Raoul Spifame's *Dicaearchiae Henrici regis christianissimi progymnasmata* (1556). A collection of 306 *arrêts* or laws supposedly composed by the reigning monarch Henry II for the just government of the kingdom, the *Dicaearchiae* was actually a jumble of thoughts on French life and institutions written by a Parisian lawyer whom contemporaries suspected of insanity and treason. Yves Jeanclos rehabilitates Spifame and his ideas by emphasizing "sa volonté profonde de réformer le royaume" (p. 10).

Spifame stressed the need to simplify the judicial process. He sought a reduction in court costs and delays, the adoption of a uniform style for trial procedure, and the supervision of judicial decisions. He also advocated the extension of royal authority into ecclesiastical and seigniorial courts.

To open access to justice he suggested "*chambres compromissaires*" for the arbitration of family arguments, and courts of equity staffed by farmers and magistrates for the resolution of agricultural disputes. In Spifame's proposals nine new chambers at the high court in Paris, two more Parlements, and twenty-four provincial privy chambers would help achieve his goals of bringing justice to Frenchmen and additional strength to the crown.

The changes Spifame sought for the judiciary revealed the limitations of his reform ideas. Spifame proposed that each judge would work a six-month semester and spend the second half of the year either at leisure or presiding on another bench. Such an arrangement, Jeanclos argues, would have undermined Spifame's suggested court reforms by continuing venality, creating 4,400 new officers, and disrupting the rapid execution of justice. The semester system would have exacerbated rather than resolved judicial problems.

Jeanclos, a legal historian at the University of Strasbourg, has done an excellent job documenting the contemporary institutional sources for Spifame's ideas. He has not, however, related Spifame's views to those of the other sixteenth-century lawyers and magistrates who also wrote on legal problems. Had he made this comparison, he would have been able to show that Spifame's treatise led the way in the emerging literature of judicial reform.

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CAROLYN C. LOUGEE. *Le Paradis des Femmes: Women, Salons, and Social Stratification in Seventeenth-Century France*. Princeton: Princeton University Press. 1976. Pp. ix, 252. \$16.50.

In *Le Paradis des Femmes* Carolyn Lougee presents an intelligent and ambitious attempt to relate the woman question in seventeenth-century France to "crucial controversies over social organization" and the "major issues of social transformation which concerned seventeenth-century Frenchmen." She further aspires to explain why the "woman question" has flared up at particular historical moments and to throw new light upon the historiographical problems of "social fusion, social mobility, and aristocratic reaction." In effect, she argues that the flurry of feminist writings in seventeenth-century France must be understood as a concerted ideological program designed to legitimize the social pretensions of the new nobility of robe and finance; that the opposition to feminism derived from conservative noble circles intent on barring the assimilation of new wealth to valid status; and that the defense of a subordinate do-

mestic role for women, as refurbished in the writings of Fénelon and Madame de Maintenon, announced that particular aristocratic reaction which sought to defend the prerogatives of the old nobility of blood not merely against the third estate, but against the recently ennobled within the second estate as well.

Lougee's defense of her thesis progresses from a discussion of feminism and antifeminism as they appeared in published works, through a social analysis of the personnel of polite society—the family background, husbands, and marriages of salon women—on to an analysis of the significance of Saint-Cyr, “the Counterinstitution,” which includes her readings of Fénelon and Madame de Maintenon as well as a picture of the social composition of Saint-Cyr. Along the way, she draws upon the methods of intellectual and social history and touches upon a wide range of topics from love, Neoplatonism, aristocratic mores, Cartesianism, and *préciosité* to domesticity, work, the attack on luxury, and the importance of family. Opinions will doubtless diverge on the extent of Lougee's success in synthesizing this broad range of material. But her systematic attempt to understand a given society's treatment of the woman question as a function of its general social concerns and to ground that treatment in the process of social change itself represents a significant contribution to the history of women, as well as a clear demonstration of its importance to a full understanding of historical process in general.

Space prohibits the full discussion that Lougee's work invites. Despite, or perhaps because of, the deceptively neat package in which her findings are presented, questions escape the confines of her framework on practically every page. Lougee seems to suggest that the centrality of the woman question in her period reflected a profound ideological readjustment within the nobility: both the pro-luxury, aristocratic “individualists” (her upwardly mobile salon ladies) and the pro-frugality, noble celebrators of domesticity incorporated strands of seventeenth-century modernity, just as both foreshadow different aspects of yet more modern feminism. Similarly, both groups drew upon older ideological traditions, particularly with respect to the idea of nobility itself. Thus both groups seem to have been engaged with a range of social and ideological concerns that Lougee fails to address directly. For example, the discussions in both camps about marriage and the appropriate social space (salon or chateau) intersected with other discussions about the nature and structure of political authority. The reassessment of the relationship between the domestic and political spheres, the preoccupation with the nature of paternal authority in both, and the relative strength-

ening of both the conjugal family and institutionalized government at the expense of lineage and clientele systems all figured prominently in the ideological and practical explorations of the period. The linkages between high culture, particularly theory, and historical process are notoriously difficult to trace. Lougee correctly insists upon the importance of women in a period of ideological transformation, but in so doing she slights the complexity of the intellectual traditions, as well as of the sociopolitical system.

If, as Lougee argues, an understanding “of the social and ideological developments of the seventeenth century of which the woman question is so central a part is indispensable for a correct interpretation of the following century and of the cataclysm which brought the *ancien régime* to a close,” then the theoretical perspective brought to bear upon those developments becomes all the more important. The growth of the state and the flowering of commercial capital produced ideological cleavages in the upper reaches of English society not so different from those Lougee describes for France. Yet, as Lougee clearly sees, Whigs and Tories cannot simply be conflated with new and old French nobles. The critical differences would seem to lie in the formal political structure on the one hand and the pervasive social relations of production on the other. In eschewing both perimeters of noble experience, Lougee undermines the extensiveness of the claims she makes for her material. Paradoxically, the similarity of rhetoric and concepts employed by her two groups emerges as forcefully as the differences. Her theorists seem to have been speaking not merely about the same set of historical circumstances, but about the same modes of conceptualization and justification. Thus the newly ennobled invoked work to sanction their acquisition of status and Fénelon and Madame de Maintenon prescribed work to legitimize female existence. The incidences of such overlap could be much extended. They suggest, in aggregate, a common rethinking of the social space, the relationship between household and polity, and the search for a new ideology of nobility. In this context, the internecine squabbles may occasionally have disguised the depth of agreement: the legal structure of estates was to be preserved at all costs.

Lougee has elegantly demonstrated the emergence of an ideology designed to legitimize recent entry to the group. She may, however, have missed two other elements in the ideological battle. First, that the reaction of the old nobility may have derived primarily from a desire to keep its own from falling out—according to Georges Duby the original reason for legal noble status. Second, that both groups, concurring in their desire to preserve

noble identity, differed in their estimates of the best ideology of nobility, understood not merely as immediate self-justification but also as a mediating hegemony to guarantee acceptance of the entire estate in the eyes of society at large. From this perspective, Lougee's insistence upon the centrality of society's perception of women to society's perception of itself may have yet broader implications than she suspects.

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HEIDI NEUENSCHWANDER-SCHINDLER. *Das Gespräch über Calvin: Frankreich, 1685–1870. Historiographische Variationen zu einem interkonfessionellen Thema.* (Basler Beiträge zur Geschichtswissenschaft, number 136.) Basel: Helbing and Lichtenhahn. 1975. Pp. vi, 258. 37 FR.

In 246 wide-ranging pages the author of this Basel dissertation surveys two hundred years of Calvin interpretation in France after 1685. As her subtitle indicates, she views Calvin interpretation in France as involving historiographical variations upon an interconfessional theme, unlike interpretations of Luther and Zwingli which lacked the interconfessional unity she finds in France. Because the usual formula "*Cuius regio eius religio*" did not hold for France, the author argues, an interconfessional dialogue developed early, although it broke apart under later cultural and political influences.

Three epochs are considered: seventeenth-century discussions reflecting the impact of the civil and religious wars; eighteenth-century interpretations influenced by the Revocation of the Edict of Nantes and new Huguenot persecutions, and most notably colored by Voltaire's Enlightenment views; and postrevolutionary nineteenth-century essays in which nationalists such as Michelet interpreted Calvin as a civilized statesman, Romantics embraced old positions, new confessional traditionalists like Capefigue revived Ultramontanism, and Protestant revivalists like d'Aubigné re-glorified Calvin.

Under the sway of Louis XIV's politics, the seventeenth-century French Catholic historians Maimbourg, Varillas, and Bossuet viewed Calvin as responsible for French civil war, while Richard Simon wrote from a scientifically detached attitude. The Protestant historians Bayle, Jurieu, Basnage, Le Clerc, and Leti extolled the sane rationality of Calvin the theologian, scholar, and biblical critic, while they did not idealize his person. Together, the two groups offered an exalted interpretative perspective on Calvin.

While her treatment of Voltaire is too brief to

satisfy the specialist, the author brings out the surprising degree to which the life and career of the Jansenist-influenced Voltaire paralleled Calvin's development, though Voltaire at the same time developed another view of faith and culture. Quite otherwise than in the age of Louis XIV, Voltaire's reduction and simplification of Calvin's *gloria dei* to *gloire de la France* reflected a powerful European reaction against two centuries of religious wars, persecution of heretics, and theological disputes. In her cool way Neuenschwander-Schindler presents a compelling view of Voltaire in continuity with her overall interpretation.

After the French Revolution, nineteenth-century Calvin interpreters in France became interested in Calvin as a statesman. A revived "*France Protestante*" emerged in the nineteenth century (although Madame de Staël's *De l'Allemagne* in 1813 reacted so far in a German direction that she recognized only one reformer, Luther!), and only in the nineteenth century did basic Calvin research and publication of sources begin in France. Under the aegis of the Romantic movement old biographies and sources by Bolsec, Desmay, de Jussie, the Galiffes, and Audin were published, providing a basis for Calvin studies more oriented toward documents than were previous writings, especially those of Voltaire.

This readable, well-researched, and subtle book is a useful addition to literature interpreting Calvin and the Reformation in modern France and Switzerland.

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TIMOTHY TACKETT. *Priest and Parish in Eighteenth-Century France: A Social and Political Study of the Curés in a Diocese of Dauphiné, 1750–1791.* Princeton: Princeton University Press. 1977. Pp. xiii, 350. \$19.50.

Timothy Tackett's regional history of the parish clergy of Gap aptly illustrates an important trend in the historiography of early modern France—a major revision of the traditional history of religious attitudes, institutions, and practices in the eighteenth century.

Because the Church's function in the *ancien régime* as keeper of records was in itself vital, it is possible in those regions where records are extant to study the clergy in a comprehensive fashion that is rare for other social groups. In some dioceses like Gap these materials have been surprisingly ignored or only partially utilized. Tackett displays both imagination and diligence in the use of archival sources. Among the records he consulted were

registers of pastoral visits, records of tax boards, parish registers, ordination rolls, declarations of revenue by clergy, and manuscript letters.

Specialists in recent years have revised dramatically the role in which the French parish priest has been cast in the scenario of eighteenth-century life. In contrast to the literary stereotype of priestly poverty and humble origins that has been perpetuated since the eighteenth century, a new picture is beginning to emerge. The curés of France appear as a true élite: they were recruited from families in the middle economic range in their communities and were sufficiently well off in their benefices despite pockets of poverty. They were often prominent in their parishes through their educational, charitable, and civic works; their spiritual ministry; and equally noteworthy, their commercial activities in farming, leasing out land, and collecting tithes.

There are essentially three sections in this study: first, the career of a curé and his role in the parish; second, the politicization and revolt of the parish clergy in Gap and Dauphiné; last, the priests and the Civil Constitution of the Clergy of 1790 and the dechristianization of the Terror.

Tackett's discoveries concerning the social derivations of the clergy of Gap correlate with those of some scholars who have studied other regions; persons recruited to the priesthood stemmed from the same general social strata as those from such wealthier and more populous dioceses as Autun, Coutances, Reims, and Le Mans. In this respect, the universal requirement that the young candidate possess a "patrimonial title" (*titre clerical*), equivalent to a benefice, to guarantee an annual income was a notable factor in imposing selectivity for the profession. Entrance to a benefice without cure of souls, as prior or canon, was even more restrictive, with recruitment entirely from families of notables (nobles, office-holders, liberal professions, self-styled "bourgeois"). Competition for benefices at Gap as elsewhere was fierce.

Although income from benefices in Gap was considerably below other regions because of the relative lack of tithe ownership, a career in the clergy could contribute to the upward social mobility of the cleric's family because of the numerous perquisites a respected curé could command and because he did not have heirs. Moreover, a pattern of family influence and structure is clear: the parish clergy of Gap was essentially a corps of younger sons, and one of four had a brother in the priesthood.

In charting fluctuations of clerical recruitment in Gap, the author notes a resemblance to trends in distant dioceses (Reims, Rouen, Autun, Rodez). In general the graph of ordinations moved up and down to peak around 1755, decline sharply

after 1766, and rebound before the Revolution. In the north of Gap, he attributes the increase of vocations after 1750 to improved economic conditions and an expanding population. In the south, in the valley of the Durance, he links ordination trends with that decline of collective piety in adjoining Provence first noted by Michel Vovelle (*Priété baroque et déchristianisation en Provence au XVIII^e siècle* [1973]), who traced the demise of Baroque ceremonial in funerals.

Tackett makes a compelling case in the second section for the uniqueness of the politicization of the curés of Gap and their organized resistance in the pre-Revolutionary period. Among the factors leading to a collective mentality were family milieu, education (especially at seminary), antagonism to and resentment of tithe owners, pastoral distance between priest and bishop, intellectual and social companionship, and frequent sessions of the cantonal conferences of priests.

The author suggests that the alienation and subsequent political activities of the Dauphiné clergy prefigured provisions of the Civil Constitution of the Clergy of 1790. Further, he believes it was the local impact of dechristianization policies during the Terror that dislocated the lives of the priests of Gap. The emphasis in these two cases seems somewhat misplaced. Though the Dauphiné curés had sought changes in the Church through appeals to civil authority they had nevertheless remained loyal to essential components of the structure of the Bourbon régime: the system of orders, privileges, corporate rights, church property, and paternalism in religious affairs. Church and State were intimately linked and mutually consultive. The curés sought to enhance their position in both by a restructuring of authority and wealth in the Church and by an extension of their powers and responsibilities as local agents of the government. But the unilateral actions of the revolutionaries—in confiscating Church property, destroying the religious orders, and, in the Civil Constitution, revamping the entire ecclesiastical structure—clashed with the practical goals of the curé movement for reform. It was not so much the utopian ideals of the clerical reformers as the excessive scope and pragmatism of their goals within the Bourbon régime that brought them to ruin during the Revolution. The elitist position of the curé was destined to be drastically altered because it was closely linked with the wealth and prestige of the Church, the two principal targets of the Church's critics before the Revolution.

Carefully researched and clearly written, this is a significant monograph. It raises fundamental questions in the history of the clergy and illuminates the way for future research. It joins company with such standard works on the clergy as Girault,

Fracard, Julia, Perouas, Greenbaum, Berthelot du Chesnay, and McManners.

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RAMON M. LEMOS. *Rousseau's Political Philosophy: An Exposition and Interpretation*. Athens: University of Georgia Press. 1977. Pp. x, 262. \$14.50.

In this first installment in a series of interpretive works on political theorists of natural law, Rousseau's arguments are carefully picked apart, tested for their coherence, pressed for their implications, and set into the body of his political philosophy as a whole. Ramon M. Lemos covers familiar ground; as in many similar expositions, the reader is led from the state of nature through the institution of private property to the association of the *Social Contract*.

The book does have self-imposed limitations which might reduce its usefulness to some readers. Lemos purposely avoids confronting other scholarly interpretations of Rousseau's thought. He confines his analysis to an examination of five of Rousseau's major works; there undoubtedly will be those who feel uncomfortable with a discussion of Rousseau's view of the social impact of religion which lacks references to the *Émile*, and with a discussion of Rousseau's belief in the "practicability of republicanism" which does not mention the works on Corsica and Poland.

While most of the exposition is clear, it seemed to this reader that Lemos occasionally muddies the often confused distinction in Rousseau's thought between sovereignty and government. It is perplexing to be told on page 71 that Rousseau believed monarchy, aristocracy, and democracy were acceptable forms of the state, then to be informed on page 195 that Rousseau considered a republic the only legitimate form of political association, when it is stated on page 131 that Rousseau's term for democracy was republicanism.

In the view of this reader, the most serious problem with the book lies in its failure to maintain its principal thesis—namely, that Rousseau as a thinker must be placed within "the Christian tradition." To Lemos this assertion implies that Rousseau believed morality and justice were more valuable than civilization and that Rousseau adhered to a certain "Christian individualism," according to which men were to be treated as ends, not as means and their souls could enjoy "the happiness their goodness merits" after bodily death. It is perhaps true that these beliefs are common to Rousseau and to Christianity. But this does not allow one to say that Rousseau can mean-

ingfully be termed a "Christian thinker": first, because these beliefs are hardly particular to the Christian tradition, and second, because Rousseau rejected many major Christian articles of faith, including the divinity of Christ. In this regard it seems illegitimate to argue, as Lemos does, that because Rousseau believed natural man could be corrupted in the state of society, Rousseau's natural man can therefore be understood to be born with original sin.

These are important failings, but they do not cancel out the book's virtues—sensitivity and patience in the analysis of one of the most intractable of political philosophers.

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JEAN-PAUL JOUBERT. *Marceau Pivert et le pivertisme: Révolutionnaires de la S.F.I.O.* Paris: Presses de la Fondation Nationale des Sciences Politiques. 1977. Pp. 295. 95 fr.

Marceau Pivert experienced a long and, by most standards, ultimately fruitless career as a socialist militant. Dynamic and outspoken in speech and in print, Pivert was less an ideological innovator than a strategist and activist, a man for whom the role of *enragé* was ever attractive. By inclination and perhaps by vocation, Pivert adopted views sharply distinct from the reformist leadership of the SFIO, and in the early 1930s championed revolutionary defeatism, sought tactics suitable for the preparation of the total conquest of power, and issued appeals for proletarian organization. Animator of the *tendance* which was self-advertised as the "Revolutionary Left," Pivert won adherents in the Paris region and in numerous *fédérations* of his party. He conceived of the Popular Front as a large step toward revolutionary fulfillment if it could mobilize masses and kindle proletarian consciousness. Harnessed only briefly in the service of the Blum government, Pivert became in 1937 and 1938 increasingly impatient with class collaboration and the politics of the Popular Front parties, so much so that, painful and dangerous to the Socialist chiefs, he was in effect driven from his party in 1938. From then until his death in 1958 he maintained his socialist commitment and credentials, but with little apparent effect.

Pivert inspired loyalties as well as hostilities, but earned more admiration than followers. Yet rather little has been written about him and his *tendance*: Daniel Guérin's *Front Populaire: Révolution manquée* (1963) is a general account by a sometime admirer, leaving Donald Baker's fine article in the *Journal of Modern History* (1971) as the only scholarly analysis. Jean-Paul Joubert's study, a labor born of

admiration, is meticulously researched, warmly sympathetic but also critical, and provides a broad narrative account of Pivert's activities, primarily in the crucial 1930s. Unfortunately it is also relentlessly tedious, written in much the same manner as older works on socialism, such as D. Ligou's. Joubert's insistence upon reporting—what someone said at what hour of the day, for example—obscures many of the more significant questions such as the impact of Pivert's stridency, his ideology, however rudimentary, and the growth and appeal of his *tendance*, particularly to younger socialists. Facts, articles, and speeches simply do not speak for themselves, and only in his conclusion does Joubert offer sustained interpretation, largely by means of a polemic against Baker. Joubert believes that Pivert's career was instructive and capable of inspiring new generations, especially as it demonstrated the need to forge new directions for the proletariat, lifting it from the morass of politics as currently practiced in Western Europe. Nonetheless, the work has value as the only full-length account of Pivert and his comrades, who played a larger role in left-wing politics than is ordinarily believed.

NATHANAEL GREENE
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ANTHONY ADAMTHWAITE. *France and the Coming of the Second World War, 1936–1939*. London: Frank Cass. 1977. Pp. xxii, 434. \$27.50.

Anthony Adamthwaite tells us in his introduction that his book "breaks new ground because it is the first full study of French foreign policy in the crucial three years preceding the outbreak of the war." Since it draws on new evidence from the French archives and from private papers, says the author, many "myths" are "exploded," and such hidden details as French pressure on the Czechs before Munich and British interference in the formation of French cabinets are "revealed for the first time." Adamthwaite also suggests that he will be advancing a revisionist view of the much-abused Georges Bonnet, and that he will be focusing sharply on the "interplay of foreign and domestic perils" in prewar France, when "many of the propertied classes went in fear of their lives," and "Left and Right took to the barricades."

Fortunately, the body of the book rises well above the level of the introduction, which reads at times like a publicity blurb. Adamthwaite settles into a highly detailed account of French diplomatic activity from 1936 onward, derived for the most part from French and British sources, but supplemented by some German materials, and by Ambassador Bullitt's reports. The story is heavily

documented (though the publisher has done his best to frustrate the reader who wants to find a given footnote, and occasionally the author cites no source at all for a challenging bit of information). On the whole, however, Adamthwaite has combed the available materials with admirable thoroughness. He also interrupts the chronological account midway with topical chapters on the various branches of the French government that dealt with foreign policy matters.

Several central themes emerge from the book. There was, in the author's view, no sharp line of division separating resisters and appeasers within the French government; everybody agreed that the Czechs must make substantial concessions to Germany. France's long-range objective throughout was an agreement with Germany—a goal that embodied "considerable idealism," but that "precluded the active defence of central Europe." Even more important to the French was the Mediterranean region, which "was given absolute priority in the years 1936–39." French policy became "much more self-assertive and independent" after Munich relieved France of the Czech burden. Franco-British relations were terribly flawed by years of bickering and suspicion, for which both sides seem to be held about equally responsible (though French leaders are in the end condemned for their "supine acceptance" of conditions laid down by London). The revisionist line on Georges Bonnet turns out to be a curiously mixed judgment; although Adamthwaite repeatedly demonstrates Bonnet's flaws of character (describing his memoirs as "a pretentious and windy farrago of half-truths and evasions, the whole salted at times by a brazen mendacity"), he asserts nevertheless that "the popular view of Bonnet as a weak-kneed, spineless individual is a gross misrepresentation," since Bonnet was sincerely motivated in his quest to preserve peace. France's military chiefs, on the other hand, are charged with "pusillanimity" for their reluctance to stop Germany by force.

Although Adamthwaite promises to deal with the domestic determinants of foreign policy, and says at the end that "the greatest failure of French governments was in internal affairs," he barely mentions, and does not develop, the social and economic issues that divided French opinion and weighed on the policy-makers. He has chosen to write diplomatic history of the traditional sort, closer to the "one-clerk-said-to-another" model than to the broader-based kind that attracts most historians these days. Within these self-imposed limitations, the author has made a useful contribution, provocative and well informed if not always fully convincing.

GORDON WRIGHT
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RENÉ RÉMOND and JANINE BOURDIN, editors. *Edouard Daladier, chef de gouvernement: avril 1938-septembre 1939*. Paris: Presses de la Fondation Nationale des Sciences Politiques. 1977. Pp. 319. 95 fr.

This is the first of two volumes of papers selected from forty-five reports to a conference held in December 1975, the purpose of which was to re-examine the period April 1938 to September 1939. The general thrust is to show that it was not a spiritless interval between the collapsed Popular Front and the onset of war, but rather a time of regeneration and resolution. The professors and actors here testifying will not accept the weary cliché that characterizes the later Third Republic as rotten, corrupt, doomed, incapable of saving itself, and so on. This is refreshing, but it is not quite proof of a generic renaissance within the nation as it moved on to the errors that occasioned military defeat. The irreducible fact of an embittered working class will not go away; there is a great deal here on the disaster of the November 30, 1938 general strike (viewed from Paris and the provinces), the determination of Reynaud, backed by Daladier, to smash it, and the bloody-minded retribution exacted by some employers. The steady falling off of parliamentary coherence and the growth of executive authority are underlined: the regime was exceptional, but its destination remains obscure. What is suggested, however, is that Daladier's temporary full powers were a natural prelude to those which, in dramatic circumstances, would be granted to Reynaud's successor.

Among the most interesting discussions are those relating to the abrogation of the forty-hour legislation, the pell-mell preparation of the Reynaud decrees, and the effectiveness of the Daladier-Reynaud tandem. Of interest, too, are the memories of such survivors as André Delmas, René Belin, Michel Debré, and Roger Gènebrier. The essays on religious policy (judged not a great success), the use of the radio, and the emergence of public opinion polls (woefully inadequate on internal policy, but exposing the myth of a PSF groundswell, and, contrary to legend, showing majority support for a firm foreign policy) are of value. Foreign policy is examined in a penetrating manner, confirming both British tutelage and its Mediterranean limits.

What do these studies rest on? The usual books and published documents, the press (still the principal French resource for contemporary history), the British papers, and the Daladier archives. The superbly catalogued Daladier collection yields many public documents, a scattering of nearly indecipherable manuscript notes, and a simply desolating mass of fragmented partial narratives he worked on almost to the end of his life in the

hope of producing memoirs. But it is limited. There is no discussion here of the closed public archives—not one word of protest from contemporary historians. Debré, nearly forty years after the event and after a career marked by some acerbity, cannot bring himself to name ministers who opposed Reynaud. This is another view of things, another world; the professors' silence and acceptance is no less remote from the world of contemporary history in the Anglo-American nations. We have no notion of how policy was made; for the influence of Paris salons on foreign policy we are referred to the wartime chronicles of Pertinax and Lazareff. You can analyze and categorize and count and speculate til the cows come home; there is no substitute for the documents. Insofar as this symposium is a success—and it is—it is because of the partial triumph of intelligence and perspicuity over the lamentable dead hand of politicians and bureaucrats, changing and unchanged from regime to regime, who consider the recent past too serious a business to be left to contemporary historians.

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JACK E. REECE. *The Bretons against France: Ethnic Minority Nationalism in Twentieth-Century Brittany*. Chapel Hill: University of North Carolina Press. 1977. Pp. xxii, 263. \$16.95.

As French writing on the subject of "ethnic minority nationalism" tends to be long on committed polemic and short on detached analysis, it is very useful to be able to turn to this dispassionate and scrupulously researched study of the constituent elements of the Breton nationalist movement. Jack E. Reece starts with a succinct demythologization of pre-twentieth-century Breton history. He then examines the predominantly right-wing, clerico-aristocratic origins of the miniscule movement for Breton regional autonomy, aimed primarily at protecting the status quo from disruptive modernizing forces. Reece describes the growth of a more radical right-wing movement in the interwar period, culminating in collaboration with Nazi Germany by the more extremist elements in the forlorn hope that this would provide the necessary aid in breaking the grip of Paris. After this unmitigated fiasco, the postwar revival of Breton nationalism took the prudent form of promoting regional, cultural, and economic development, which, after some successes in the Fourth Republic, was mastered by Gaullism in the early 1960s. The historical survey concludes with an examination of the revival of Breton nationalism of a more explicitly political and sometimes violent kind, though the

most vigorous components are now predominantly and explicitly left-wing. Throughout, Reece stresses the role of Breton intellectuals as the dynamic driving Breton particularism.

The author has made a good, readable summary of a complex story, though there are some notable gaps in the bibliography. While he mentions their important role in the 1950s and 1960s, Reece does not appear to have consulted the mammoth *Debout Bretagne* of Michel Philipponneau or *La Région, Pour un État Moderne* by Joseph Martray, both published in 1970, which represent the views of leading protagonists of alternative routes to greater Breton autonomy and development. Nor does he mention the leading Marxist analysis of *La Question Bretonne*, published in 1975 by Renaud Dulong.

The main problem with this book, however, is that the author does not provide a clear statement of his interpretative thesis. While in the introduction he seems to give greater weight to the Berger-Heiseler thesis that ethnic cleavages have displaced class-based cleavages, in the concluding chapter he dwells at greater length upon Hechter's "internal colonialism" thesis, although he prefates references to it with words like "alleged" and "so-called." Here is an example of how he hedges his bets on Hechter's hypothesis: "This argument simply squares with what *appears to be* the objective reality of the situation" (p. 228–29, italics added). The "internal colonial" interpretation has been supported by the findings of scholars such as Eugen Weber, notably in the final chapter of his engrossing *Peasants into Frenchmen*, which contains many illuminating remarks about Brittany as the French colony par excellence. So, while Reece is to be congratulated upon having assembled some of the materials for an interpretation, his failure to arrive at a firm standpoint leaves the reader rather in the air about what to make of it all.

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CLAUDIO SÁNCHEZ-ALBORNOZ. *Spain, a Historical Enigma*. In two volumes. Translated by COLETTE JOLY DEES and DAVID SVEN REHER. (Biblioteca de Hispanismo, number 2.) Madrid: Fundación Universitaria Española. 1975. Pp. 1,265.

Writing in 1960 Jaime Vicens Vives declared that to write a history of Spain without taking into account the debate between Américo Castro and Claudio Sánchez-Albornoz "would be inconceivable." While some historians would not say that today, there is still general agreement that the debate has advanced our understanding of Spain. Castro's *La realidad histórica de España*, a revised

version of *España en su historia* (1948), was translated into English in 1954 as *The Structure of Spanish History*; a partial, revised version, *The Spaniards, An Introduction to Their History*, appeared in 1971. Sánchez-Albornoz's reply to *España en su historia*, *España, un enigma histórico*, has appeared in several editions since 1956.

Both Castro and Sánchez-Albornoz possess complete confidence in their ability to dominate the whole of Spanish history. This confidence often proves unjustified. Sánchez-Albornoz is primarily known as an institutional historian, Castro as a historian of literature and a philologist. Sánchez-Albornoz is more at home than Castro in the Visigothic period and the history of the early Christian north after the Islamic invasion of 711. Castro in the later Middle Ages. Sánchez-Albornoz can establish, against Castro, the need to take pre-Islamic Spain into consideration when discussing later developments. When he ventures onto literary ground he is less fortunate. Compare his sketchy treatment of Don Juan Manuel on Holy War (p. 302) with Castro, *The Spaniards*, pages 495–98. Neither author has a firm grasp of Arabic sources. This gap is particularly evident in Sánchez-Albornoz's constant use both here and in *La España musulmana*—his well-known anthology of sources—of old and unreliable translations. (See P. Chalmers, in *Hispania*, 35 [1975].) The unreliable sources Sánchez-Albornoz possesses he employs with evident bias. For him, the fact that Spanish Muslims were mainly of Spanish descent constitutes proof that they "were unable to transmit a functional Arabian structure which they lacked" (p. 20). On this basis there would be few Muslims outside the Arabian peninsula (and few Christians outside Palestine). Nor can the idea that Spanish Islam "lived for the most part from the pre-Muslim Western heritage" (p. 181) be entertained by anyone aware either of the extreme poverty of the actual heritage received by Islam in Spain or of the indifference to it displayed by leading Islamic thinkers.

When Sánchez-Albornoz comes to the other non-Christian Spanish community, the Jews, the aim is again polemical, to prove that "there is nothing more opposed to the authentically Hebrew than the essentially Spanish" (p. 769). To this end the abundant evidence for harmonious relationships between Jews and Christians before 1348 is silenced. The fact that Yitzhak Baer's great *History of Spanish Jewry*, published in English in 1961–66, is not used in the most recent edition of Sánchez-Albornoz's work, is revealing. If it had been, the author might have had to discard the theory (originated by Castro) of the Jewish origin of the idea of the Spanish Inquisition. His bias prevents Sánchez-Albornoz from realizing basic

facts about medieval Spain, for instance that the weakness of medieval Spanish scholastic philosophy and science is linked to the strength of the Judaeo-Islamic learned tradition *outside* the comparatively insignificant universities.

For Sánchez-Albornoz the history of Spain is a "long adventure, the Reconquest and Repopulation of the national land, from Covadonga (722) to Granada (1492)" (p. 29). To see the Reconquest as "the key to the history of Spain" (p. 623) inevitably leads to the depreciation of the peoples "reconquered." The deep influence of Islam on the society and culture of Spain is ignored in favor of the continuity of an underlying *homo hispanus*.

The main habitat of this "Spanish man" is Castile, though he often comes originally from Asturias or the Basque country. Other regions of the peninsula suffer in comparison. Generalizations broadly true of Castile, such as the lack of a native bourgeoisie, are applied to Spain (p. 621) with apparent unawareness of the central position of the bourgeoisie in the Crown of Aragon, especially Catalonia, Valencia, and the Balearics.

Sánchez-Albornoz's book contains many interesting observations but it ignores or underestimates vast areas of Spanish life—non-Castilian Spain, Islam, Judaism. The failure of this massive attempt of traditional Castilian institutional history to controvert—or assimilate—Castro's book is instructive. Castro's work has many gaps. Like Sánchez-Albornoz, Castro concentrates overmuch on Castile. But he has opened new ways to understanding. The interaction, violent or peaceful, of Christians, Muslims, and Jews, on which Castro insists, is not the only important feature of Spanish medieval history, nor does the abrupt termination of this relationship in itself explain modern Spain, but it is one of the main keys to Spanish history. It can no longer be forgotten or explained away.

Sánchez-Albornoz's book has until now been unavailable in English. The present translation is so inadequate as hardly to change this situation. Abundant misprints, continual grammatical errors, confusion over ancient and medieval names, are less serious than sheer misunderstanding of the original. Sánchez-Albornoz is made responsible for very strange statements. We have "Corsica in constant fight with Geneva" (p. 69) (read Genoa). The manuscripts of Beatus' commentary on the Apocalypse (in Spanish *beatos*) become "pious persons" (p. 53), rendering the comparison here between Castile and Galicia unintelligible. On page 94 hunger dominating medieval Spain becomes "attachment to the service of a lord." We are told (p. 297) that Spanish religiosity was "remote" in 711; the original states it was "*remoto y arraigado*" (long and deeply rooted). "There were not any [*sic*] Jewish functionaries in the courts of the

Christian Spanish kings" (p. 622); Sánchez-Albornoz wrote the reverse. Other errors are entertaining. We have "the runner Orosious [*sic*]" (p. 221), for "the restless Orosius" (the historian). Ferdinand refers in 1481 to the Jews as "our coffins" (p. 841), instead of "coffers" (*coffres*). This list could be prolonged indefinitely. There is no attempt to translate the many Latin and French quotations in the original. Notes, lacking there, are supplied only for pp. 138–80. Those who wish to read the authentic text of Sánchez-Albornoz will still have to do so in Spanish.

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A. W. LOVETT. *Philip II and Mateo Vázquez de Leca: The Government of Spain (1572–1592)*. (Travaux d'Humanisme et Renaissance, number 155.) Geneva: Librairie Droz. 1977. Pp. 214.

From J. H. Elliott's Cambridge seminar of the mid-1960s came a splendid group of young scholars of imperial Spain. The most outstanding so far seem to be Richard Kagan, Geoffrey Parker, and A. W. Lovett. Lovett has not only given us fine articles on the administration of Philip II's monarchy, but also the excitement of an academic "feud" with Parker. More than anyone else, Lovett has shown that what often seemed Philip II's dissimulation was really confusion, and that at the court of Madrid policy was too often determined by men who, however well-meaning and intelligent, can best be described as limited.

Lovett here gives us a most important study (based largely on scattered archival collections that have seldom been used coherently) of Mateo Vázquez de Leca, from 1572 until his death in 1591 Philip II's personal secretary. Vázquez dealt in one way or another with virtually every issue of importance to the monarchy, and many more.

After introducing Vázquez and describing how he garnered his office, Lovett proceeds to deal topically with the problems faced by Philip, Vázquez, and their associates: the Netherlands, finances, government reorganization, the Indies, the Church, the Moriscos, and Aragon.

One must, however, be well informed about the personalities and problems of the reign to appreciate this work, since Lovett focuses chiefly on how issues were handled, almost on a day-to-day basis. The major issues comprehended dozens of attendant issues. Often one finds oneself *in medias res*, getting little background and sometimes no clear view of outcomes. Of course, ongoing problems often seem to have had no clear outcomes, but Lovett might well have offered something in sum-

mary at the end of chapters and of the book. So much is illuminating—the sense of how government worked, the chapter on finances—that one regrets having to quibble. But this is a book for the advanced student of the reign, not the beginner.

Lovett provides a good portrait of Vázquez and makes some provocative statements about Philip's psychology, but does not amplify.

There is no bibliography, although some footnotes provide bibliographical commentary. The index seems unfortunately inadequate for so rich a work.

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LAURA RODRÍGUEZ DIAZ. *Reforma e Ilustración en la España del siglo XVIII: Pedro Rodríguez de Campomanes*. (Publicaciones de la Fundación Universitaria Española: Monografías, number 15.) Madrid: Fundación Universitaria Española. 1975. Pp. 345.

LUIS SIERRA NAVA-LASA. *El Cardenal Lorenzana y la Ilustración*. (Publicaciones de la Fundación Universitaria Española: Monografías, number 11.) Madrid: Fundación Universitaria Española. 1975. Pp. 355.

The eighteenth century is now the favorite among historians of Spain. Rich in documentation and filled with the drama of the monarchy's efforts to revive the kingdom's ailing economy and its colonial empire in the New World, the "enlightened" reign of Charles III (1759–88) has in particular attracted considerable attention. In North America the works of George Addy, William Callahan, Richard Herr, and David Ringrose have helped to revise much of our thinking about this period, and in Spain itself a new generation of historians, many trained in the tradition of the *Annales* school, has embarked on important new research, particularly in the realm of economic and social history. Scholars such as Gonzalo Anes and Miguel Artola, both of whom head influential historical seminars in Madrid, have shown the way, and in their footsteps have followed a host of younger historians whose works are only now beginning to become known in this country. Both of the biographies currently under review are the product of this new wave of interest among Spanish historians in eighteenth century studies, even though both are cast within the traditional guise of the Enlightenment rather than the vocabulary of the *Annales* school.

Laura Rodríguez, a young historian trained in Oxford and Madrid, is already known in this country for her article "The Spanish Riots of 1766" (*Past and Present* [May, 1973]). This study reap-

pears in slightly altered form as chapter six in what otherwise purports to be a political biography of that well-known and often studied figure of the Spanish Enlightenment, Pedro Rodríguez de Campomanes. Spain's version of a French physiocrat, Campomanes established a reputation as a reformer while working as a lawyer in the 1750s in Madrid. When he was named *fiscal* of the Royal Council of Castile in 1762, his proposals for agricultural development, educational reform, the disentanglement of church property, free trade, and an end to special privileges of every kind foreshadowed the liberal program of the nineteenth century. Unfortunately, the portion of this book which examines Campomanes' position on these and other questions, although offering some fresh archival material, does little to alter our image of the man or his thought.

The author's discussion of the riots of 1766, which occupies the last third of the book, is another matter. Ostensibly, a detailed analysis of these uprisings is included in this biography because they are interpreted as "a reaction to the reformist policies" (p. 10) that Campomanes helped initiate. Accordingly, Madrid's famous "*motín de Esquilache*" is viewed not as a truly popular riot but one planned and manipulated from above, principally by conservative (but unnamed) ecclesiastics and noblemen opposed to the Enlightenment. This is an intriguing, although not very original hypothesis, but aside from some anonymous (and possibly untrustworthy) accounts of the uprising, Rodríguez offers little proof to support her point of view. Much more convincing is the description of the provincial riots which followed in the wake of the Madrid uprising. These are depicted as popular, spontaneous outbreaks brought on by increases in the price of bread and the belief that officials were manipulating grain supplies for personal profit. Campomanes' connection to these popular riots is never made clear, but the author makes sense when she compares these *motines* with other European "food-riots" of the eighteenth century. Her notion that Spain did not experience more disturbances of this type because of the charitable activities of a wealthy Church is, however, not substantiated. Castile, it is true, recorded no such riots between the Andalusian "*alteraciones*" of the 1650s and those which Rodríguez describes, but to attribute this quiescence solely to a Church which itself suffered from the kingdom's economic decline is somewhat naive. Explanations for the tranquility of this epoch must also be sought in the kingdom's relative freedom from population pressure until late in the eighteenth century as well as in the strong yet paternalistic nature of municipal government throughout Castile.

Luis Sierra Nava-Lasa's biography of Francisco Antonio Lorenzana y Butrón (1722–1804) is, like Rodríguez's study, also concerned with aspects of Spain's "*ilustración*," but as it adopts a more limited approach to its subject the result is quite different. This is a narrowly focused, somewhat adulatory biography which does little to explain the economy and society in which Lorenzana lived. As the first of a projected multivolume work this volume deals only with Lorenzana's early career, beginning with his years as a grammarian in a Benedictine monastery, and then as a law student in Salamanca and Valladolid, vicar general of Toledo, bishop of Plasencia, and, finally, archbishop of Mexico (1766–72). The author's treatment of this prelate's life is richly documented, frequently with extracts of Lorenzana's own writings, although the tedious baroque style in which it is written detracts from what otherwise would be the first comprehensive study of this influential figure.

In other works Lorenzana has been branded as a "reactionary," mainly for his efforts in the 1790s when, in his capacity as Inquisitor General, he spearheaded an attack on those who endangered the privileged position of the Catholic Church in Spain. This study of his early life depicts him in an entirely different light. He emerges a dedicated reformer who endeavored to infuse the Church with a spirit of charity and inject high standards of discipline and morality among the clergy, both secular and religious. As archbishop of Mexico, for example, he introduced a new catechism for the use of the natives, reformed the religious houses of New Spain, dispatched missionaries to convert Indians living in semicolonized border regions, and convoked (in 1771) the fourth council of the Mexican clergy in order to institute a number of important reforms. Lorenzana is also portrayed as a man of culture and learning who, among various projects, prepared editions of a gothic missal, numerous medical and other scientific treatises, and Hernán Cortés' *History of New Spain*, in addition to collecting Aztec artifacts and assembling a collection of *mestizo* crucifixes. Yet it is also clear that Lorenzana was no radical; he aimed at strengthening the Church, not displacing it, and his reforms were aimed at restoring the clergy to a position of intellectual and moral leadership within the Hispanic world. It is no surprise then that he reacted adversely in the 1790s to the threat posed by the French Revolution to the established institutions of Spain. Like most of Spain's "*luces*," Campomanes included, he was prepared to revitalize Spanish society, not alter it.

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MICHAEL E. BURKE. *The Royal College of San Carlos: Surgery and Spanish Medical Reform in the Late Eighteenth Century*. Durham, N.C.: Duke University Press. 1977. Pp. xv, 215. \$10.75.

In the eighteenth century certain reforming trends became apparent in medicine. Two major areas for change were the way in which the medical profession was organized and the structure and substance of medical education. The functions and status of each of the three groups—physicians, surgeons, and apothecaries—in the hierarchical division of medical practitioners came under review and, in medical training, the role of the basic sciences and clinical experience emerged as primary issues. Michael E. Burke makes a contribution to knowledge of these debates by considering medical reform in Spain in the eighteenth and early nineteenth century. The vehicle for his discussion is the history of the Royal College of San Carlos, a training institution for surgeons established in 1787 in Madrid, which placed emphasis on clinical experience and a knowledge of the natural sciences and anatomy in surgical education. As elsewhere in Europe, the struggle of the surgeons in Spain to upgrade their education or professional status and escape the domination of the physicians is found to play a key role in the development of a new medical outlook at the end of the century. Institutions outside the universities proved to be the centers for dissemination of new medical information and innovative techniques.

In describing this development, Burke endeavors to go beyond the presentation of an institutional evolution and examine the context in which it came about. He discusses the efforts of the Crown and of enlightened reformers to diminish corporative power and reform education in eighteenth-century Spain and the effects this had on medicine. By such an analysis Burke gives his work a much broader compass. The early date of the attempts to bring medicine under royal control in Spain and the unusual character of the Royal *Protomedicato*, a body which controlled licensing of practice for physicians, surgeons, and apothecaries in all parts of Spain and was also responsible for guarding the public health, are facts which should be noted by those interested in the activities of centralizing monarchies in relation to medicine.

Burke's study is based on extensive and obviously careful archival research. It is valuable in making detailed information on Spanish medicine available to a wider audience of historians. Very little has been written in English on this subject and period, and Spanish historians of medicine have generally not attempted to relate medicine and society in the way achieved by Burke. This

work makes it possible for useful and informative comparisons to be made with developments in other European countries and Great Britain and adds much to our overall knowledge of professional organization and education in the eighteenth century.

CAROLINE HANNAWAY
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BEATRIZ ROMERO BLANCO. *José del Castillo y Ayensa, humanista y diplomático (1795-1861)*. Pamplona: Ediciones Universidad de Navarra. 1977. Pp. 318.

He was "not a brilliant man but doubtless was a useful one" (p. 308). So concludes Beatriz Romero Blanco of José del Castillo y Ayensa—nineteenth-century Spanish diplomat, senator, and Hellenist. In truth, Castillo emerges from this study as a limited, workaday *moderado* politician whose outstanding characteristics were his loyalty to Queen Mother María Cristina and his pursuit of personal financial security.

Making extensive use of state and private archives to explore the life and character of Castillo, Romero Blanco seeks to supplement recent monographs by F. Suárez and J. Pérez de la Alhama which deal with church-state relations and to cast new light on politics in both Madrid and Rome.

Although Castillo's career is traced in its entirety, fully two-thirds of the book concerns the abortive agreement (*Convenio*) Castillo negotiated with Rome in 1845. The author investigates both the negotiations themselves and subsequent attempts to fix blame for the *Convenio*'s failure. She demonstrates that Castillo's actions were contrary neither to the ministers' stipulated bases for agreement nor to their subsequent instructions. Blame for the failure rather should lie with Madrid, whose orders were confused and frequently at odds with the agreed-upon bases. For his part, Castillo worked to resolve the inconsistencies in such a way as to preserve the original intent of the government. Romero Blanco concurs in the explanation offered by Suárez, Pérez de la Alhama, and others for Madrid's rejection of the agreement: the *moderado* ministers distrusted the *Convenio* article which tied papal recognition of revolutionary land sales to decorous financial support for cult and clergy. If Rome decided financial arrangements were inadequate, would it refuse to recognize the sales? This fear was reinforced by an angry public which sharply questioned the government's ability to safeguard national dignity. Castillo, as the man on the spot in Rome, provided a handy scapegoat around whose neck charges of failure could be hung.

Although the interpretation is not new, Romero Blanco does provide valuable supporting detail and thoroughly acquits Castillo of contemporary charges of failure to follow instructions. One misses, however, exploration of pressures acting on the *moderados* at home (for instance, the impact of public disturbances evoked by imposition of a new and highly unpopular tax system). The author points to stabilization of the *moderado* order as crucial in explaining the Concordat with Rome in 1851 but does not investigate the effect *lack* of order had on failure in 1845.

Overall, the study is considerably better in providing political than personal information. Partly because the organization is strictly chronological, little of the human side of the man is revealed. The author's tendency is to narrate rather than to analyze. The result is useful in the detail it offers but weak in the characterization it shapes or new interpretations it presents.

NANCY A. ROSENBLATT
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HERBERT RUTLEDGE SOUTHWORTH. *Guernica! Guernica! A Study of Journalism, Diplomacy, Propaganda, and History*. Berkeley and Los Angeles: University of California Press. 1977. Pp. xxvi, 537. \$19.95.

Few incidents of the Spanish Civil War have been so deeply etched on the memories and consciences of men as the aerial bombing of Guernica on April 26, 1937. Picasso's artistic talent gave expression to what indignant people the world over feared: that the destruction and death wreaked upon a Basque town's unarmed civilians had become an accepted battle tactic and was a harbinger of things to come in other wars. Oddly, the events which occurred in Guernica, as well as the ideas which motivated those events, are still not widely known. Moreover, the record has been difficult to reconstruct because of contradictory evidence, the presentation of which has sparked much debate and contention. One has only to read the carefully balanced, dry, and somewhat incomplete account of Guernica in Hugh Thomas' latest, revised edition of *The Spanish Civil War* (1977) to perceive the handicaps under which historians have been laboring.

Herbert Southworth has undertaken the herculean task of addressing himself to almost all facets of the Guernica "problem": what actually happened at Guernica and why; and how and why various institutions and individuals have tirelessly suppressed or distorted the truth for forty years. The second element of the problem took him into a maze which is reflected in the subtitle of his tome: "a study of journalism, diplomacy, propaganda,

and history." The author emerges from that labyrinth with credit, for he has unearthed significant and fresh information, and skillfully knitted together the many tangled threads of evidence. Southworth, whose career has embraced writing, government service, and entrepreneurial work in radio, has long had a reputation as an implacable foe of the Franco regime and as a spirited controversialist. *Guernica! Guernica!*, in any case, rests on an objective use of numerous and diverse sources.

Space permits consideration of only a few of Southworth's conclusions. He buries the old story that Guernica was put to the torch by retreating Republicans and builds a fair case for the contention that the Nationalist high command desired to raze the town. The goal was to terrorize the Basques and in so doing snap their will to resist at Bilbao, where Mola feared a long and costly siege. Southworth believes that the air raid, conducted by German pilots of the Condor Legion, was carried out without the German Air Ministry's approval, and that Spanish and German officials in Salamanca indulged in further collusion to justify themselves to a Berlin coping with the worldwide sense of outrage over Guernica. The lies emitted by the Nationalists concerning Guernica reflected a desire to maintain the dignity and appeal of the Franco "crusade." It was hard to give such a notion currency during a merciless campaign against the Catholic and conservative Basques of Spain. Apparently the Nationalists were particularly concerned about the erosion of their support among Catholics abroad, especially wavering liberal Catholics. The desire to retain the support of sectors of Catholic and conservative opinion in foreign countries is also adduced to explain, in part, the continuation of the lies into recent years.

Southworth, who resides in France, was puzzled and dismayed by the bland French response to Guernica. His probe of this is impressive. Unpublished evidence indicates that Havas, the French news agency, manipulated news of Guernica to favor the Nationalists. It did so partly out of fear that reportage of the truth would lead to the expulsion of the agency's correspondents from the insurgents' zone, which would have badly damaged Havas' lucrative position as a disseminator of news about Spain in Latin America. Other, highly competitive agencies were seeking to fill Havas' shoes there. Moreover, in France Havas was subsidized by the Quai d'Orsay, which intervened in the agency's circulation of information about Guernica to prevent French popular opinion from reacting violently and demanding aid for the Spanish Republic. The Quai also wanted to prevent a possible deterioration of relations with Germany over the issue.

Southworth's book has some flaws. It is sometimes repetitious in theme and turn of phrase. The section dealing with the submissive mendaciousness of what he calls the "neo-Franquista" school of writers is mildly polemical. Unfortunately, the author did not seek out all accessible German sources. He states that most of the Condor Legion records were destroyed (p. 504), but scholars familiar with the Bundesarchiv in Freiburg im Breisgau assert that the bulk of Legion records are there. Had Southworth used these he might have been able to answer the questions of whether or not the Germans engaged in systematic terrorization at Guernica and whether or not they were testing new tactics and equipment there. Apparently Southworth did not examine the lengthy Plocher manuscript, "The German Air Force in the Spanish War, 1936-1939," in the National Archives, Washington, although he knows of it. Plocher, who was in Spain, blames the Guernica raid on the Italians! As Southworth has admitted, "the Guernica controversy has not ended" (p. 397). It will not terminate until Spanish, German, and other documents are brought to light and certain people with knowledge interviewed.

These shortcomings by no means detract from the merit of this fine book. It is recommended not only to those interested in Spanish and European history, but also to students of propaganda and journalism.

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MIKE L. SAMSON. *Population Mobility in the Netherlands, 1880-1910: A Case Study of Wisch in the Achterhoek*. (Acta Universitatis Upsaliensis, Studia Historica Upsaliensia, number 87.) Uppsala: Historiska Institutionen vid Uppsala Universitet; distributed by Almqvist and Wiksell International, Stockholm. 1977. Pp. 180. 53 KR.

Gelderland is one of the five easternmost provinces of the Netherlands contiguous with Germany. One of its districts, the Achterhoek (literally, "the back corner"), has been much studied for the remnants of traditional practices that long since disappeared from the provinces of North and South Holland along the coast. Wisch, the subject of this work, is a township of the Achterhoek.

Several threads of recent research have been woven into the study, but not merely derivatively. That the country's arable land is, broadly speaking, either sand or clay suggested quite different patterns of agricultural development to such earlier analysts as E. W. Hofstee. The clay areas, naturally more fertile, shifted from smallholders to

large commercial enterprises; but when artificial fertilizers became available at the end of the last century, the extension of agriculture in the sand areas was mostly through an increasing number of small farms, with a concomitant rise in fertility. Using detailed data for Wisch, Mike L. Samson offers some useful corrections to this hypothesis; in particular, the changes came earlier than analyses based on broader data indicated.

The introduction of fertilizers to the region around Wisch coincided with a growth of textile factories, iron foundries, and brick-making plants. Thus there was a simultaneous expansion of both agriculture and industry, the prior out-migration was cut, and natality went up. Many earlier studies of Dutch fertility, particularly a pioneering work by F. van Heek, concentrated on the Catholic-Calvinist differences in average family size. Samson notes that the Catholics, who in this province were a minority, showed also a far greater propensity to move out. Since at that time the incidence of interreligious marriages was close to nil, young Catholics could hardly find a spouse without going south to the predominantly Catholic provinces. Much of the migration, back and forth, was to the adjacent region of Germany, where developing industry and the new middle class provided opportunities for young men fresh off the farm and young girls employable as housemaids. Though across an international border, this migration, as the author points out, had many of the characteristics ordinarily associated in migration theory with internal movements.

The main source of Samson's study was the repository of population registers, in which are recorded not only vital statistics but migratory shifts. Thus it was possible to link general trends in the country's economy and population with more specific and detailed changes in this area. These population registers, potentially a most valuable basis for research in historical demography, have been used far less than they deserve; and one hopes that this initiative will be followed up.

Originally a doctoral dissertation at the University of Uppsala, the book was published as the sixty-fifth in a series in demographic-geographic history. It is a modest performance; apart from appended tables and bibliography, the text runs to only 139 pages. Within its compass, it is worth the attention of anyone interested in pursuing Europe's economic-demographic history in the late nineteenth century through first-rate case studies. As an American professor who has all but despaired of getting students to master even one foreign language, I note in passing that this author not only lists in his bibliography works in Dutch, Swedish, English, German, and French but has written in an English that, despite an occasional

unidiomatic turn of phrase, compares well in style with the average native dissertation I have seen.

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SIGURDUR A. MAGNÚSSON. *Northern Sphinx: Iceland and the Icelanders from the Settlement to the Present*. Montreal: McGill-Queen's University Press. 1977. Pp. viii, 261. \$12.00.

Sigurdur A. Magnússon is a poet and world traveler who has long worked in journalism, but also as teacher, translator, and tourist guide. All of this shows in his present volume. While well grounded in Icelandic history, he is, not surprisingly, drawn by his nation's literary monuments. He discusses the political aspect more briefly and with less enthusiasm, and his view here can only be described as cautious and somewhat old-fashioned. The work is not meant to be history in the customary sense, as is made clear in the foreword: "This book is intended as a general introduction to Iceland, past and present, with special reference to cultural trends in its historical development."

The coverage of Icelandic history is uneven. The first four (out of ten) chapters (pp. 9-100) deal with the Icelandic Commonwealth (874-1262), though mythology and literary history loom larger than political events and trends. Only a single chapter (pp. 101-38) is spent on the period 1262-1918; its heading, "Foreign Domination and Natural Catastrophe," announces a romantic orientation. But, in all fairness, it should be pointed out that up-to-date research on the era is limited, which handicaps those wishing to take a fresh tack in a short survey.

The last five chapters, all subdivided, focus on the Icelandic Republic (since 1944). It is in this general section that the perspective shifts to a description of society, with light thrown on assorted peculiar and comical features; the author knows what intrigues, what prompts questions, and what evokes smiles.

The history of the modern period very much takes a back seat to a description of Icelandic society. For this, Magnússon uses *Iceland 874-1974* published by the Central Bank of Iceland; few sources could have been more reliable. A most valuable contribution is the extensive select bibliography of works in English relating to Iceland. Regrettably, factual errors occur—and misprints are all too common. This reviewer for the most part approves of how Icelandic proper names are handled; the discussion of this problem in the foreword gives good insight into certain linguistic aspects and the patronymic convention.

Finally, it is difficult to avoid wishing that the

book had been somewhat longer, that more topics had been introduced, and that the coverage had been in different proportions. Nevertheless, the work fills a gap. And it is to be hoped that a second edition—expanded and revised—will be forthcoming. And some stylistic improvements could perhaps be made, to appeal to readers whose attachment to English is as strong as poet Sigurdur A. Magnússon's to his mother tongue.

BERGSTEINN JÓNSSON
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PAUL WAEBER. *La formation du canton de Genève, 1814-1816*. Geneva: The author. 1974. Pp. 391.

Any book published *chez l'auteur*, and with the help of public subsidies, whets a reviewer's curiosity. But anyone who expects to discover something remarkable, too controversial for an academic publisher yet too erudite for a commercial publisher, will be disappointed by this eminently solid piece of mildly revisionist diplomatic history. Paul Waeber had already summarized his thesis in a contribution to the collaborative *Histoire de Genève*, edited by P. Guichonnet (1974), before publishing this book. His main argument is that the government of post-Napoleonic Geneva was extremely suspicious of, and hesitant to press for, significant enlargements of its territory, although relatively eager (insofar as "old-Genévans" ever can be) to join the Swiss Confederation. Thus it was not simply Talleyrand's evil machinations, but also the ambivalence of the restored Republic of Geneva, that was responsible for the small size and military indefensibility of the new Swiss canton admitted in 1815, whose borders were definitively fixed the following year. Even the hero of Genevan and Swiss diplomacy at the Congress of Vienna, Pictet de Rochemont, emerges from Waeber's carefully detailed narrative as an inconsistent and sometimes unenthusiastic expansionist; like his Genevan colleagues, he believed "*s'il fallait choisir entre 230.000 ressortissants de toutes couleurs, ou 33.000 seulement, avec adjonction à la Suisse, je préférerais ce petit nombre, qui nous aurait laissé notre nationalité et tous les moyens de lustre dont notre histoire littéraire, religieuse et morale fait foi*" (p. 124).

This book offers a necessary corrective to the overly saccharine view of the events of 1814-15 which is still commemorated by most Genevans and most Swiss, but its findings do not justify the idiosyncratic *boutade* in its conclusion against university-based histories. Ironically, a book which successfully demonstrates the pettiness of Genevan diplomatic goals at a critical juncture of the city's history unintentionally offers yet another illustration of this tendency. Waeber could have done an

even better job by going beyond "the most essential manuscript sources at Geneva and Turin" (p. 374), beyond the copious writings of the *vieux-Genévois*, to explore the intentions and activities of the other parties intimately concerned with the creation of the Canton of Geneva, particularly the local notables of Savoy and the Pays de Gex.

E. WILLIAM MONTER
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FRANZISKA ADELMANN. *Dr. Dietrich von Plieningen zu Schaubegg*. (Ludwigsburger Geschichtsblätter, number 28). Ludwigsburg: J. Aigner. 1976. Pp. 139.

Among the most intriguing of questions in the history of early modern Germany is the reciprocal relationship between political action and the encounter with classical antiquity. The life of Dietrich von Plieningen, the subject of this short but meticulous biography, makes a particularly good case in point. A minor Swabian noble by origin, Plieningen was trained in Pavia and Ferrara as a career specialist in governmental administration. He took a law degree, studied Greek, befriended humanist scholars who called him "Plinius," and perfected his command of rhetoric. In 1482 he joined the government of the Rhine Palatinate, serving under Chancellor Johann von Dalberg, whose friendship he had gained in Pavia. In Heidelberg Plieningen divided his time between diplomatic activities and literature, for the cultivation of which the Palatine court provided an excellent setting. Of special significance was his support of another of his friends from Pavia and Ferrara, Rudolf Agricola, whose *De inventione dialectica* seems to have been completed at the urging of Plieningen.

In 1495 Plieningen became one of the sixteen assessors at the Imperial Chamber Court founded that year; four years later he moved to Bavaria where, for the last two decades of his life, he pursued two careers in scholarship and politics. He translated Sallust, Cicero, Pliny, and Seneca into German and undertook the reform of Bavarian civil and procedural law. Unlike other German participants in the reception of Roman law, however, Plieningen threw his weight to the side of the Bavarian Estates in their struggle against an arrogant and unstable ruler. His finest moment as a politician came at a meeting of the Bavarian Landtag in 1514 when, as spokesman for the territorial Estates, he faced Duke Wilhelm down with a stern lecture on proper princely behavior and just government. As reflected in his speeches on this occasion (their salient passages are given in the notes to this book), Plieningen's political principles appear

to be derived from, and are certainly supported by, his reading of the classics, especially of the writers he had translated. As a further buttress for the defense of traditional liberties he collected and published the historical privileges of the Bavarian Estates so as to make them available for use in future contentions between ruler and subjects.

The author, an amateur historian, has constructed an admirably clear and reliable outline of Plieninger's life. Her aims are modest. She hopes to stimulate interest in the man, and to be superseded as his biographer. To this end she invites other scholars to make use of her large collection of xeroxed Plieninger documents. It is to be hoped that her invitation will be accepted. There is still much to be learned about this interesting man, especially the link between the political and scholarly strands of his career. That they nourished and reinforced each other seems beyond question. But the demonstration that this was so remains to be accomplished.

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ROGER A. JOHNSON, editor. *Psychohistory and Religion: The Case of Young Man Luther*. Philadelphia: Fortress Press. 1977. Pp. vii. 198. \$12.95.

This collection of essays is a ragged effort to deal with Erik Erikson's *Young Man Luther*. Some of the essays have already appeared in print. Roland Bainton, always a severe critic of Erikson, flails him for misinterpreting Luther's childhood, especially his relationship to his mother and father. Hans Luther was not the ogre Erikson makes him. Luther's theology was not a reaction to a brutal and alcoholic father but a response to the terrifying theology of the age.

Lewis Spitz sings treble to Bainton's bass, following Bainton point by point and using the same evidence to prove the same things. Though Spitz lards his work with references to other scholars in the field, many of them relate only marginally to the subject at hand, serving only to make a conspicuous display of erudition. Unfortunately, imagination is less easy to demonstrate.

The most interesting contribution is that by the editor, Roger A. Johnson, "Psychohistory as Religious Narrative: The Demonic Role of Hans Luther in Erikson's Saga of Human Evolution." Johnson argues that Erikson's work is something like a theology of human existence. Hans Luther is an ideological construct, necessary for Erikson but not really the Hans Luther of flesh and blood. Hans thus becomes a mythical figure, and historian Erikson is made to look as bad as Bainton and

Spitz say he is. Yet Erikson's book becomes a valid interpretation of a general human experience.

Unresolved conflicts abound here. The psychologist begins with the faith that certain things in human nature are always the same. People who experience roughly similar things (for example, brutal fathers) in any age will react in roughly similar ways (silence for years and then explosion) that will be translated into forms (theology, for example) appropriate to the culture. The historian, on the other hand, is persuaded that peculiar cultures create peculiar people and that analogies between past and present are dangerous.

So Erikson studies modern children and imagines that he has a hold on Luther. Bainton and Spitz study the sixteenth century and cry that Erikson is wrong, that Luther is to be explained by his place in a unique moment of history rather than by personal details of universal human experience, that he was shaped by the great waves of a culture and not by such petty things as bowel movements and fear of his parents.

Erikson leaves us wishing he did not treat his evidence like a valet to be ordered about according to the plans the master has for that day. Bainton and Spitz, who simply cannot bear to draw any negative conclusions about Luther, leave us wishing they would quit trying to excuse or explain away Luther's wrath, vulgarity, and hatreds. Contrary to both of them, Luther was overwhelmingly more scatological in print than most other educated men of his age. They, too, should try to use these traits, as well as theology, to grasp the man.

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HEINRICH VOLBERG. *Deutsche Kolonialbestrebungen in Südamerika nach dem Dreissigjährigen Kriege, insbesondere die Bemühungen von Johann Joachim Becher*. Cologne: Böhlau-Verlag. 1977. Pp. 223. DM 40.

In this book, a Cologne dissertation, Heinrich Volberg presents an overview of schemes advanced in Germany in the seventeenth century for colonization in South America. He examines Courland's tiny settlement on Tobago in the 1650s and the series of proposals for South American colonial companies discussed in various German courts in the 1660s. Volberg is primarily concerned with the attempt in 1669 by the entrepreneur Johann Joachim Becher to promote a colony in Guiana, to be established by the Count of Hanau with the cooperation of the Dutch West Indies Company. Like most of the other ventures, the Hanau scheme collapsed before a single ship was sent out. Its main significance lies in the involvement of Becher.

Becher was one of the creators of German cameralist economic theory; his colonial projects were one means by which his economic ideas became widely known. A self-made man of obscure origins, Becher spent his life promoting various business enterprises in Germany and the Netherlands. The Hanau project was one of his last. Becher persuaded the West Indies Company to grant part of Guiana to the profligate Count of Hanau, a temporary colonial enthusiast. Becher hoped by this means to establish a company to monopolize the sale of tropical products within the empire. The scheme shortly fell through because of political strife in Hanau and lack of imperial support for a clearly impractical idea.

Volberg's book reveals the extremely superficial nature of German seventeenth-century colonialism and the inclination of German princely governments to consider projects for economic expansion with little regard for economic realities. It demonstrates the crucial role of the Netherlands in German economic life and also a tendency, even in Dutch imperialist planning, toward fuzzy thinking. The book is based on primary materials, although the main source is Becher's published *Political Discourse*. One is inclined to wonder whether Volberg's topic is sufficiently important to warrant an entire book. Becher's Hanau project would have been an adequate subject for an article, but the book as it stands is filled out with accounts of much less significant ventures, and even with a summary of the history of the West Indies Company. This additional material might be useful to those needing an overview of German colonialism in the seventeenth century. Because the author concentrates on South American projects, however, he naturally leaves out of consideration the few successful German endeavors elsewhere, such as Brandenburg's West African slaving operations.

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HANNS GROSS. *Empire and Sovereignty: A History of the Public Law Literature in the Holy Roman Empire, 1599–1804*. Chicago: University of Chicago Press. 1975. Pp. xxx, 541. \$19.00.

This book reviews public law theory during the period when it was a university discipline taught separately from private law. Most scholars will not feel out of place with the subject matter of the earliest public lawyers, since they worked within the traditions of familiar medieval legal commentators and humanist theorists. To public lawyers up to the Thirty Years War, the empire was still the final world-state described in the Book of

Daniel, a channel of divine justice to mortal men. German institutions were explained by redefining them in Roman terms, often with fruitful results, as when the electoral capitulations were understood as a form of the *lex regia*.

In the middle of the seventeenth century, however, the shock of the Thirty Years War led many scholars to reject the use of Italo-Latin concepts as anachronistic. The empire was increasingly seen as a German monarchy or commonwealth, understandable in native terms. In this period foreign political writers such as Bodin, Hotman, and Hobbes had considerable impact on German thinkers. The complexities of the actual German polity were faced squarely for the first time, and the restrictions on imperial power were interpreted as guarantees of a special German liberty. The elimination of antique interpretations also opened the way for a German concept of *raison d'état*. The last phase in the evolution of public law was reached when the empire became simply the guarantor of all rights and privileges possessed by society, so that structural analysis was replaced by an inventory of accumulated practice, as in Johann Jacob Moser. This "positive public law" was, in part, a symptom of the failure of the German constitution to make sense in terms of social needs by the eighteenth century. As Hegel acidly declared at the end, further political progress could only take place through the demise of the empire: "Let justice be done, let Germany perish" (p. 479).

Hanns Gross' book is a remarkably sympathetic reconstruction of a dead legal tradition which makes public law intelligible in its own terms. There is a great deal of material here to fill in the gaps in the descriptions of German constitutional thought sketched by Klaus Epstein and Leonard Krieger. The author's preoccupation with detail for its own sake, however, makes for slow and difficult reading. This problem is made worse by the fact that the present version simply reproduces a typescript originally prepared for microfiche in 1973. The only change from the microfiche edition is a retyped index, so the bibliography is already out of date. In all, though, a very useful book has at last been made available to a wider range of potential readers.

STEVEN W. ROWAN
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REINHARD RIESE. *Die Hochschule auf dem Wege zum wissenschaftlichen Grossbetrieb: Die Universität Heidelberg und das badische Hochschulwesen, 1860–1914*. (Schriftenreihe des Arbeitskreises für moderne Sozialgeschichte, number 19.) Stuttgart: Ernst Klett Verlag. 1977. Pp. 414. DM 64.

Despite the student unrest of the late 1960s and the subsequent attempts at academic reform, West German historians have contributed surprisingly little to the discussion about the crisis of their universities. Occupied with the success of Nazi propaganda among students, scholars have often neglected the origins of Weimar's structural problems and solutions in the Second Empire.

One notable exception is the Conze dissertation of Reinhard Riese, which probes the causes and consequences of the transformation of German universities between 1860 and 1914 by analyzing the example of Heidelberg, the cultural policy of Baden, and the trends of the Reich. His social-historical thesis focuses on the emergence of the *Grossbetrieb der Wissenschaft* (Harnack) which made for "a steadily increasing discrepancy between the neo-humanist ideal entrenched in constitution and ideology, and the actual structure of the university" (p. 13). The enrollment boom and the explosion of science rendered it "more and more impossible for the university to reconcile the tasks of humanistic *Menschenbildung* (cultivation), professional training, and scholarly research which confronted it" (p. 14). Based on the files of the university archive in Heidelberg and those of the Cultural Ministry of Baden, Riese's book presents a careful and largely successful account of the problems of innovation in the statutes (e.g., the division of the philosophical faculty into humanistic and natural sciences), the teaching faculty (new chairs as well as the increase of *Nichtordnarien*, i.e., those below full professorial rank), and in seminars, research institutes, and clinical facilities. In spite of the unquestioned leadership of German universities around the turn of the century, Riese paradoxically concludes: "German educational and social structure, the immobility of the university, the federalist fragmentation of higher education, and the ossification of individual institutions . . . delayed many necessary reforms so long" that they exacted a painful price half a century later (p. 337).

Despite presenting a wealth of well-organized information, Riese's dissertation suffers from a lack of conceptual focus (e.g., in part three dealing with Badensian university policy he offers either redundant material or a survey of the literature in only tangentially related areas). His social analysis of the causes of the enrollment boom remains largely barren, because he shies away from a prosopography based on the matriculation registers, which despite some problems, can clarify the changing composition of the student body, as has been shown by a number of historians including this reviewer. Though listing the university's triple aim of cultivation, professional training, and scholarly research in the introduction, Riese does not

contribute to our understanding of the political implications of higher education (i.e., the controversial issue of socialization and academic authoritarianism). He is not always successful in explaining why universities changed in order to accommodate increasing numbers of students and scholarly specialities while rhetorically clinging to neohumanism and preserving the power of the *Ordinarius*. Finally, the issue of modernization remains ambiguous because he never spells out what is positive or negative about scientific specialization, mammoth institutes, industrial influence on research or what a "modern" university constitution or ethos might be. Hence, Riese's impressive dissertation adds more to our knowledge about the development of the mass university than to our understanding of its problems.

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ERWIN GATZ, editor: *Akten der Fuldaer Bischofskonferenz*. Volume 1, 1871-1877. (Veröffentlichungen der Kommission für Zeitgeschichte, number 22.) Mainz: Matthias-Grünwald-Verlag. 1977. Pp. cxxiii, 789. DM 154.

The Kulturkampf remains among the least well understood problems of nineteenth-century German history. For the most part what has been written is so contradictory and prejudiced that even now—one hundred years later—the issues remain controversial and, in many respects, obscure. A major reason for this confusion is the fragmented nature of the historical evidence. The record of the Kulturkampf to this very day is widely dispersed, much of it hidden away in inaccessible depositories.

Until recently the political archives of the German and the Prussian governments constituted the main evidential source for most writings about the Kulturkampf. But inasmuch as access to ecclesiastical archives, both in Germany and in the Vatican, is now permitted on a larger scale, this imbalance in the availability of sources is gradually being corrected. The publication of this volume represents another step in this direction; it makes available the record of the proceedings of the Prussian Bishops' Conferences between July 1871 and August 1887. The record of these conclaves, which normally met in the episcopal city of Fulda, is based on nearly five hundred documents found in the diocesan archives of Cologne, Breslau, Limburg, Münster, and Trier. Additional material from the German Foreign Office for the period 1882-87 amplifies the collection. These documents are essential for the study of the Kulturkampf be-

cause they provide new factual material about such issues as the reception of papal infallibility in Germany, the Braunsberg school conflict, Otto von Bismarck's repressive "May laws," the negotiations between Berlin and the Vatican which led to the dismantling of the *Kulturkampf* legislation, and even the degree to which, after 1882, the government was kept informed of what until then had been the secret deliberations at the conferences in Fulda. But these materials hold no genuine surprises, and they in no way compel a radical revision of the traditional view of the *Kulturkampf*. The chief value of this collection lies instead in the more precise insights it affords into the reaction of the Prussian hierarchy to the government's *Kulturkampf* legislation.

Indispensable though this volume is, it is nevertheless impossible to find in its documents either a complete picture of the *Kulturkampf* or an entirely accurate understanding of the nature and character of that conflict. While the behavior of the Prussian episcopate is obviously of crucial interest to the historian of the *Kulturkampf*, there is a danger of losing sight of the larger issues involved in that dispute through excessive concentration on the activities of these high churchmen and on the detailed circumstances which enabled Bismarck to make gains at the expense of the Roman Church. At its heart, the *Kulturkampf* was the outgrowth of Bismarck's efforts to prevent the parliamentarization of German political life. To understand this dimension of the *Kulturkampf*, it is necessary to go beyond the narrow confines of the church-state conflict, to see its issues within the general context of Imperial Germany's politics, and to reconstruct Bismarck's motives and aims from his actions and the general framework of his policy. This caveat aside, the appearance of this volume helps clear the ground for the comprehensive and definitive account of the *Kulturkampf* which is still lacking and which constitutes an important lacuna in the literature about Bismarckian Germany.

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SERGIO PISTONE. *Ludwig Dehio*. (Gli storici, number 8.) Naples: Guida Editori. 1977. Pp. 255. L. 3,800.

Ludwig Dehio (1888–1963) was a noted German historian, archivist, and editor of the *Historische Zeitschrift* (1949–56). He is best known for *Germany and World Politics in the Twentieth Century* (1959) and *The Precarious Balance* (1962). As Sergio Pistone argues in the first long study of Dehio, he was also an important revisionist and weighty critic of the Rankean heritage in German historical thought.

In the wake of "the German catastrophe," Dehio subjected the European system of great-power politics, the balance of power, *raison d'état*, war, and peace—and Germany's role in all—to a sobering critique.

Pistone argues that Dehio perceived what others failed to grasp. Unlike the unreflective neo-Rankeans, he saw that the European state system had grown feeble in the nineteenth century. Unlike Gerhard Ritter and other defenders of the Prussian tradition, he regarded German policy under William II, the Weimar Republic, and Hitler as fundamentally continuous and hegemonial in nature. Unlike Fritz Fischer and others who took up this theme of continuity, however, Dehio refused to succumb to moralistic denunciations of one-sided German "guilt." The unleashing of the international anarchy always potentially present in great-power competition, not just the demonic personality of Hitler or some mysterious poison in German "national character," brought about the political collapse of Europe.

Pistone's book is a detailed and admiring analysis of these themes and others found in Dehio's works and disagreements with other historians. Moving chronologically, Pistone discusses Dehio's Rankean heritage and his rejection of parts of it, the two major works mentioned above, the disputes with Ritter and Fischer, and Dehio's acceptance of European integration under American protection as the only remaining alternative to the defunct system of European sovereign states.

Pistone mentions, but hardly develops, the unusual personal evolution of Dehio. Born into a distinguished, partly Jewish Prussian family, Dehio was a patriotic front-line officer and supporter of the annexationist and conservative *Vaterlandspartei* in World War I as well as a supporter of the German Nationalists during the Weimar period. After the Second World War, he converted to Catholicism and supported Adenauer's efforts to integrate West Germany into an Atlantic community. Pistone skirts the interesting relationship between the historian's personal experience (especially under Nazism) and his altered state of historical consciousness, which led to his most provocative work. Another weakness is that Pistone overdraws the liberation of Dehio from the "Rankean" heritage and overlooks the universalistic and cosmopolitan strains common to both Ranke and Dehio.

The author admirably elevates Dehio's thoughts to a *weltanschauung* of systematic proportions and rarely points out their flaws. Nevertheless, he has produced a useful introduction to Dehio and to many of the large categorical theories of German historical scholarship since Ranke.

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RISTO ROPPONEN. *Die russische Gefahr: Das Verhalten der öffentlichen Meinung Deutschlands und Österreich-Ungarns gegenüber der Aussenpolitik Russlands in der Zeit zwischen dem Frieden von Portsmouth und dem Ausbruch des Ersten Weltkriegs.* (Historiallisia Tutkimuksia, number 100.) Helsinki: Suomen Historiallinen Seura. 1976. Pp. 220.

Although the recent debate over the Central Powers' role in precipitating World War I has been so protracted and complex as to defy precise summation in a few words, it can be very roughly perceived in terms of aggressiveness (the Fischer school) and anxiety (the Ritter school). This book falls generally into the latter category but, nonetheless, suggests that the motives of both arrogance and fear may have applied.

The work focuses on the responses of the German and Austro-Hungarian publics to "the Russian danger" between 1906 and 1914. Although written by a Finn and published in Finland, the book observes the German scholarly formula of beginning with a thorough discussion of the state of research on the subject, methodology, use of sources, and analysis of the German and Austro-Hungarian press in general. It is based on a broad use of primary and secondary documents, particularly the German and Austro-Hungarian press, and its style is clear though undistinguished, perhaps as a result of translation. After examining the major thrusts of Russian foreign policy, Risto Ropponen traces the generally increasing German and Austro-Hungarian fears of Russia from 1906, when they were at a low ebb because of Russia's defeat in the war with Japan, to 1914, when they were at a high point. Russophobia burst forth in Germany during the Bosnian Crisis (1908), with a widespread "fear of encirclement" resulting in increased pressure for augmented armaments. Contemporary Austro-Hungarian concern became even more intense during the Balkan Wars (1912-13) and sparked a heated reaction from the Vienna press, fear for the existence of the Dual Monarchy, and demands for increased arms. Ropponen regards these fears as genuine and stimulated by Russian activism in the Balkans and increased armaments on the eve of the war. Yet he feels Russophobia was exploited by leaders of the Central Powers to win passage of arms bills. Likewise extremists—above all the Pan-Germans—used the Russian danger to agitate for a more aggressive foreign policy. While not accepting the Pan-German cloth whole, many Germans—as represented by the Conservatives, National Liberals, and Roman Catholic Center Party—favored an increase in German world power and were anti-Russian. Despite their opposition to imperialism and war, the Social Democratic Party's middle and right wings accepted increased military expenditures

out of fear of the Russian danger and an economic depression. Russophobia increased during the spring of 1914, but public opinion in neither Germany nor Austria-Hungary united against Russia until the last week of the July Crisis. Ropponen thus offers a conventional description of what is generally known but alters its emphasis.

While providing an adequate summary of Austro-Hungarian and German public attitudes toward Russia during this period, the book does not deepen our understanding of the workings of public opinion. Like most studies, the book equates public opinion with the press (p. 18) but later suggests that the press influences public opinion (p. 204) and grants that the press is an inadequate measure of mass views (p. 205). Furthermore the relationship between the press and official foreign policy remains unclear to Ropponen (p. 199), although he points out the notable general parallelism between the two, a parallelism which becomes virtual congruity by the end of the July Crisis.

The book is more suggestive in its placing of the Russian danger within the context of German and Austro-Hungarian assumptions. Ropponen concludes that, in the sense of a plan of attack in the immediate future, "an acute threatening danger from the Russian side to the security of Germany did not exist" (p. 206). The real danger lay in Germany's strategic location between its opponents and Austria-Hungary's Balkan interests. Ropponen sees Germany's great power status as less secure than that of France, Russia, or Great Britain; sensing this, Germany sought security through an alternately offensive and defensive policy which reinforced its isolation and necessitated increased arms and dependence on the Austro-Hungarian alliance. The Russian menace was thus less a reality, or a Machiavellian creation of extremists, than a consequence of international circumstances.

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KURT DÜWELL. *Deutschlands auswärtige Kulturpolitik, 1918-1932: Grundlinien und Dokumente.* Cologne: Böhlau Verlag. 1976. Pp. xii, 402. DM 72.

Kurt Düwell's meticulously researched book adds another chapter to the "continuity" debate in German foreign policy by depicting the Weimar Republic's cultural diplomacy as a departure from the traditional subordination of efforts in this field to the exigencies of *Machtpolitik*. Before 1914 most German cultural programs abroad, such as the operation of German schools and religious missions, academic exchanges, and the dissemination of German publications, served as vehicles to en-

hance the Second Reich's economic expansion, colonial ambitions, and international standing. During World War I Germany's cultural contacts with other countries became exclusively propagandistic in intent. In the wake of the defeats Germany suffered on the battlefields and at Versailles, the Weimar Republic, Düwell contends, mobilized its cultural resources in lieu of military and economic power to redeem the tarnished German image in the world community.

The Foreign Office's Culture Department, which was established in 1919, gradually evolved a concept of culture as an autonomous national asset for promoting international harmony, as well as for gaining recognition for German artistic, intellectual, and technological achievements. Düwell maintains that this new orientation reflected not only a realistic appraisal of Germany's precarious status, but also the influence of the bourgeois specialists who were recruited instead of aristocratic members of the diplomatic corps to staff the Culture Department. The spirit of reconciliation, which characterized many of the endeavors of the Culture Department, paralleled and complemented Stresemann's "politics of fulfillment" in the second half of the twenties. In that period exhibitions of German paintings, showings of German movies, and performances of German musicians and acting companies helped restore Germany's prestige in other nations. The Goethe Institute program and the *Deutscher Akademischer Austauschdienst* still stand as monuments to this pacific phase of German cultural diplomacy which abruptly ended with the Nazi seizure of power.

Düwell also delineates the numerous instances in which cultural affairs continued to be exploited for ulterior political purposes. Yet the examples he presents easily could lead one to conclude that such cases represented the rule rather than the exception. Indeed, the Culture Department's most extensive program involved the protection, coordination, and partial subsidization of the various schools, churches, hospitals, and organizations which fostered and preserved the German identity of the *Auslandsdeutschen*. Although the Culture Department confined its activities in this sensitive area to strengthening the cultural ties between these Germans and the fatherland, it in effect sustained their hope for an eventual *Anschluss*. In this regard the Weimar Republic's foreign *Kulturpolitik* mirrored Stresemann's particular brand of revisionism, which pursued traditional German goals in a less bellicose manner. The alleged break with the past was, at best, incomplete.

Nevertheless, Düwell warns against judging the Weimar Republic too harshly for its lapses into prewar modes of cultural diplomacy. After all, he argues, they must be placed within the inter-

national context of this period. France, for example, had mounted a campaign of "cultural imperialism" that was far more massive and overtly jingoistic than its German counterpart. Thus, the German Foreign Office often based its cultural initiatives on the precedents set by the French. Düwell also reminds his readers that the *Auslandsdeutschen* encountered mandatory programs designed to assimilate them in some of their host countries. The Weimar Republic could hardly ignore their plight when Article 113 of its constitution guaranteed the cultural autonomy of minorities in Germany. Furthermore, a number of nations boycotted German participation in international conferences dealing with scientific and academic matters. Even after Germany had joined the League of Nations, its entry into League agencies like the International Research Council continued to be denied. Consequently, the Foreign Office faced the dilemma of asserting Germany's rehabilitation without aggravating existing Germanophobia.

Aside from the light Düwell's monograph sheds on a hitherto neglected topic, it is also a valuable research tool. Its appendixes contain important documents relating to the development of Germany's foreign cultural policies. These include Karl Lamprecht's definition of *Kulturpolitik*, the Foreign Office's secret memorandum of 1914 ordering the politicization of German schools abroad, and a series of circulars on foreign cultural affairs issued by Stresemann and Curtius.

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PAUL KLUKE. *Die Stiftungsuniversität Frankfurt am Main, 1914-1932*. Frankfurt am Main: Verlag von Waldemar Kramer. 1972. Pp. 593. DM 48.

This is a detailed history of the University of Frankfurt from its founding immediately prior to the First World War through its maturation into an institution of high standing during the years of the Weimar Republic. Paul Kluge is clearly sympathetic to his subject, shows considerable admiration for those who contributed to the university's establishment and growth, and is particularly appreciative of its early record for being socially tolerant and intellectually progressive. Beyond that, however, this is not an interpretive study, but a straightforward institutional history, organized chronologically with topical subdivisions, and written in a lucid narrative style. It contains a wealth of information that is of interest to cultural, political, and social historians because the new University of Frankfurt, like other universities, was constantly and sometimes deeply affected by what

took place in German society. The narrative is skillfully set in that larger context, though internal university matters are always the center of attention.

Kluge patiently discusses policy deliberations, funding problems, debates and decisions on academic programs, professorial appointments, composition of the student body, the establishment of special institutes, and many related points. He notes that from its beginning the University of Frankfurt differed somewhat from existing German universities. It was not founded by state action, but through private and municipal initiatives. This unique feature became less pronounced, however, as financial distress following the First World War meant that the state of Prussia had to assume responsibility for much of the university's support. The "Blossom Time" (*Blütejahre*) of the late 1920s, marked by adequate financing, success in academic appointments, and the flourishing of such affiliated undertakings as the *Institut für Sozialforschung*, was too quickly followed by internal political disruptions and a new institutional insecurity that arose out of Germany's political instability and the world economic crisis.

Unfortunately, Kluge drops his account abruptly with 1932; his cut-off date is absolute. In an extremely disappointing two-paragraph "Closing Remark" he sidesteps all the burning historical issues about the relationship of German universities to National Socialism and thus, no matter how large and how informative, this book leaves one with a profound sense of incompleteness.

VERNON LIDTKE
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ULRICH KLUGE. *Soldatenräte und Revolution: Studien zur Militärpolitik in Deutschland, 1918/19*. (Kritische Studien zur Geschichtswissenschaft, number 14.) Göttingen: Vandenhoeck and Ruprecht. 1975. Pp. 518.

REINHARD RÜRUP, editor. *Arbeiter- und Soldatenräte im rheinisch-westfälischen Industriegebiet: Studien zur Geschichte der Revolution, 1918/19*. Wuppertal: Peter Hammer Verlag. 1975. Pp. 403.

One of the major debates to emerge from the vast amount of literature on the Weimar Republic that has appeared in the past twenty years concerns the validity of the "third way" thesis. Was there a viable, democratic alternative to the policy of co-operation with the defeated empire's conservative elites that the republic's new Social Democratic leaders adopted in 1918, or, as the Majority Socialists argued, was the only alternative chaos and/or a violent social revolution? Focusing on the work-

ers' and soldiers' councils that sprang up throughout Germany in November 1918, defenders of the third way thesis have concluded not only that the leaders of the new republic greatly overestimated the councils' radicalism, but that the councils could have been used to control and democratize vital institutions which, taken over in largely unreconstructed form, later became staging grounds for antirepublican activity. Although considerable research has been done on the council movement, its two components have not received equal treatment. Most studies have concentrated on the workers' councils and have dealt with the soldiers' councils only peripherally if at all. In view of the vital role played by the latter, the need for a detailed study of the soldiers' councils has long been recognized. This need has been admirably filled by Ulrich Kluge's *Soldatenräte und Revolution*.

The book, which draws upon a wealth of local archival material, is concerned with three major problem complexes: the origins, nature, and goals of the soldiers' councils; their measures to provide armed forces for the protection of the republic; the overall development of republican military policy from November until the passage of the Provisional Reichswehr Law in March 1919, which, in Kluge's view, effectively terminated the chances for substantive military reform.

In his detailed account of the origins and goals of the soldiers' councils, Kluge demonstrates their fundamental moderation and builds a convincing case for the argument that they had definite potential as agents for the creation of a democratic armed force loyal to the republic. The councils were formed not by radicals, but by war-weary troops who feared (not without reason) that the attempts of the Max von Baden government to end the war were being sabotaged by German military leaders. The soldiers' councils aided the withdrawal and demobilization of German military forces and created an improvised but adequate system of domestic security forces and border defense units that, Kluge argues, provided a viable alternative to the Free Corps. In political matters the soldiers' councils steadfastly supported the Council of People's Representatives. Their revolutionary aspirations were directed primarily to the building of a military system that corresponded to the new democratic conditions created by the revolution. Only when it became clear that the Ebert-Scheidemann government was not going to meet these expectations did the soldiers' councils become radicalized.

By early December 1918 a precarious equilibrium had been established between the councils and the Supreme Command (OHL). The balance of power was held by the government. Whichever side it supported would determine the character of

Germany's postwar military establishment. By the end of December the government had opted for the OHL. Kluge provides an excellent, detailed analysis of the struggle between the councils and the die-hard opponents of military reform in the OHL, in which he outlines the goals, tactics, strengths, and weaknesses of both sides. In tracing the government's shift from a position of relative neutrality to one of support for the OHL, Kluge systematically develops the alternatives that were open to it and the ways in which it failed to exploit them. One of the book's most interesting contributions is to show how the abortive Volkswehr project enjoyed the support not only of the councils but of important elements within the Prussian War Ministry. Had the government worked with the councils and played off the more progressive elements within the officer corps against the unbending opponents of reform, Kluge maintains, a loyal, more democratic army could have been created.

Instead, the government sided with the Supreme Command, allowing the OHL, as Kluge puts it, to transform itself from a "*militärtechnische Liquidationsinstitution*" into an "*innenpolitische Ordnungsmacht*." This provided the officer corps with the power base it needed to vanquish its opponents. The main responsibility for this and the subsequent failure to provide the republic with a suitably democratic military system, Kluge concludes, rests with the Majority Socialist leadership of the Council of People's Representatives. Yet while critical of Ebert and the Majority Socialists, his study is balanced and unpolemical. He repeatedly notes and takes into account the crucial, fatal misperception upon which so many other mistakes were based: the belief that the old order had totally collapsed, that fears of a counterrevolution were groundless, and that the offers of cooperation and declarations of loyalty proffered by the fallen empire's administrative and military elites comprised a genuine recognition of the new, democratic order rather than, as it turned out, a temporary tactical retreat.

Arbeiter- und Soldatenräte im rheinisch-westfälischen Industriegebiet is a collection of studies of the council movement in Germany's industrial heartland, the Ruhr. Two essays by Ulrich Kluge describe the growth of soldiers' councils in the area and the activities of the *Generalsoldatenrat* in Münster. The three remaining essays by Hans-Ulrich Knies, Irmgard Steinisch, and Inge Marssolek detail the history of the council movement respectively in Wuppertal (Elberfeld-Barmen), Mülheim, and Dortmund.

In his introduction, Reinhard Rürup notes that with the rehabilitation of the councils and the discrediting of the earlier view that they were dominated by radicals, were incompetent, etc., the

danger has arisen that the councils' "revolutionary substance," i.e., the desire for substantive socioeconomic change that they embodied, might begin to be overlooked. The five essays affirm the councils' "revolutionary substance" while confirming the view that they acted responsibly and were essentially moderate in character.

The book contains much that is useful and informative. The two essays by Kluge provide a detailed picture of the soldiers' councils in Rhineland-Westphalia and reaffirm the findings of his larger work reviewed above. The studies by Knies, Steinisch, and Marssolek represent not only a geographical but a political cross-section: in Wuppertal there was a rough parity between Independent and Majority Socialists; in Mülheim the Independents were dominant; in Dortmund, the Majority Socialists. The differing balance of power between the two socialist factions largely determined the fate of the council movement, e.g. in Mülheim the Independents were able to maintain the councils as viable organizations for some time, whereas in Dortmund the Majority Socialists effectively limited their power. One of the most interesting themes that emerges from the three studies is the extent to which prewar and wartime developments affected the postwar constellation of power and thus the nature and fate of the council movement in the three cities.

The overall effect of the five essays is to reinforce the conclusions reached by Eberhard Kolb and Peter von Oertzen in their larger studies of the council movement. And yet, if they tend to confirm the general picture of the *Rätebewegung* that has emerged in recent years, they also demonstrate its diversity. Local studies, especially when they are as intelligently conceived and thoroughly researched as these, remind us of the powerful impact of local conditions in shaping events in periods of general turmoil and serve as useful correctives to oversimplification of the complex series of events that took place in Germany in 1918-19.

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GÖTZ BERGANDER. *Dresden im Luftkrieg*. Cologne: Böhlau Verlag. 1977. Pp. xv, 341. DM 58.

German historians and popular reviewers have recently been passing the word that Götz Bergander's *Dresden im Luftkrieg* is an engrossing and significant book. And indeed it is. Of course, any revisionist volume that entered an area occupied by David Irving would be of interest now that the author of the *Destruction of Dresden* has advanced

the novel contention that Adolf Hitler was a composite of the three blind mice. But Bergander's book is much more than a lucky fringe shot in the latest Hitler controversy. Relying on German, British, and American documents, he has cleared away many issues which have long hovered around the Allied air raids of February 1945. Bergander demonstrates that the number of displaced persons in the city at the time of the attacks was approximately 200,000, a far cry from the frequently mentioned figure of half a million. Similarly, he shows that the total killed in the raids was around 35,000, at least 100,000 less than the death count commonly cited by writers as diverse as Kurt Vonnegut and Hans Jacobsen. Two more Dresden myths fall by the wayside as Bergander proves that Irving's blood-curdling tales of phosphorous bombs and murderous sweeps by Allied fighter aircraft are both fantasies. The Anglo-Americans used no phosphorous bombs on Dresden, and the escort fighters only strafed ground targets during their return journey to the United Kingdom, not in the bombing area.

These points would be sufficient in themselves to make this study worthwhile, but Bergander also throws light on the broader subject of the Allied air war against Germany. His careful examination of Nazi sources enables him to go beyond the traditional two-dimensional accounts which feature Allied attack policies and the consequent slaughter of German civilians. In *Dresden im Luftkrieg* we follow the Allied raids in precise detail, but we also see them in the context of Nazi counteraction. Consequently the devastation and mass death in Dresden—and elsewhere—emerges as the joint product of Allied ruthlessness and the inefficiency and callousness of Hitler and his aides. In Dresden, thousands died because Gauleiter Mutschmann was short-sighted and incompetent; elsewhere in Germany, tens of thousands perished because Hitler chose to arm the Wehrmacht at the expense of civilians who were left nearly defenseless to bleed their way through the air attacks. This does not free the Allies from the moral responsibility for the kind of war they waged, but it leaves only German civilians as helpless victims, not the men who directed the Nazi system.

This is only one of many cogent points which Bergander makes about the strategic air offensive. When reading the book, one might occasionally wish that the author had omitted some details about the raiding forces and used the space to give us even more such insights. But Bergander is writing the history of the whole air war over Dresden and he has to balance description and evaluation. By and large he is very successful and the resulting volume should be interesting reading for Americans as well as Germans. In fact, he has done his

work so deftly that it seems fitting to turn a tired cliché on its head. Years ago, Americans liked to ask whether we would do as badly as the Germans did if we ever faced something similar to the Nazi challenge. Now, we might wonder if any of us would do as well as Bergander has done if we had to produce a fair, comprehensive, and challenging account of the destruction of our own native city.

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PETER CHRISTIAN LUDZ. *Die DDR zwischen Ost und West: Politische Analysen 1961 bis 1976*. Munich: Verlag C. H. Beck. 1977. Pp. 366. DM 19.80.

In this book Peter C. Ludz, one of West Germany's leading experts on East Germany, disclaims any intention of providing a comprehensive portrayal of the German Democratic Republic (DDR) over the past fifteen years. Instead, he poses specific questions on a number of issues: the decision-making process, the social structure of the DDR elite, the shifts of emphasis in foreign and domestic policy, the fixed and changing weak spots in the DDR power structure, and the like. The result is, nevertheless, a very thorough study of some of the most crucial phases in the development of the DDR since the building of the Berlin Wall in 1961.

Professor Ludz takes due note of the DDR's special position, owing to its economic pre-eminence in COMECON, its long-standing ideological fidelity to the USSR, and its often contradictory relationship with West Germany. The book gives an excellent account of the economic reforms of the 1960s, especially the important industrial price reforms under NÖS. Ulbricht's own definition of NÖS in 1964 reveals that he did not view the DDR economic reforms as mere imitation of the Liberman model, but an example in their own right, worthy of imitation throughout COMECON. Where Czechoslovakia had failed under Novotny, Ulbricht proposed to succeed in the DDR. Ulbricht's overbearing attitude toward the Soviets, based on his economic gains, may, in Ludz' view, have contributed to his sudden demise of 1971. In this interpretation Ulbricht would appear to have changed from socialist "*Musterknebe*" to socialist parvenu.

In analyzing the nexus of DDR economic problems, DDR-COMECON and DDR-Soviet trade relations, the author convincingly illustrates the excessive foreign trade obligations of the DDR in the Communist bloc. Apel's suicide of 1965 is examined from this as well as from several other view-points. Taking note of the DDR's activist role in COMECON, Ludz throws some interesting

light on projects for further political integration between COMECON and the USSR, such as have been discussed in the writings of Gunther Kohlmeier and Otto Finger in the DDR and by Butenko and Senin in the USSR.

The strongest part of the book is that which concerns the Honecker era since May 1971. The book provides extensive treatment of the social-economic, ideological, domestic, and foreign issues of the DDR between the 8th and 9th SED Congresses. Questions of Politburo and Central Committee personnel are interpreted both in terms of continuity or change of policy and Honecker's own power base among SED district secretaries and the veterans of the Youth Movement (FDJ). The 1976 SED program is explained as an attempt of the party to broaden its popular appeal. The reader will note with interest the growing significance of the Central Committee institutes as consultative organs of the Politburo of the SED, a development which finds its counterpart in the USSR.

There are, in Ludz' work, occasional problems of organization and repetition. Also, in the otherwise excellent analysis of DDR-BRD relations it might have been possible to offer a broader historical perspective of SPD-SED relations and to mention the relevant views of such SPD leaders as Brandt and Wehner. Otherwise, it would appear most desirable to have a work such as this available for a broader, English-speaking audience through an early translation.

ALEXANDER RUDHART
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GERHARD FLOREY. *Geschichte der Salzburger Protestanten und ihrer Emigration, 1731/32*. (Studien und Texte zur Kirchengeschichte und Geschichte. First Series, number 2.) Vienna: Verlag Hermann Böhlau. 1977. Pp. 276. DM 56.

In 1731, Leopold Anton von Firmian, prince-archbishop of Salzburg, having failed to convert his twenty-thousand-odd Protestant subjects to Catholicism, ordered the expulsion of the heretics. The Protestant powers were unable to restrain Firmian and his ardent chancellor, Cristani di Rallo, and so, after many brutalities and formalities, the Lutheran mountaineers took to the highroads of Europe. Emperor Charles VI, convinced that the Lutherans were "rebels" and thus not protected by the Treaty of Westphalia, sent dragons to assist the archbishop.

Several hundred Salzburgers died along the way to exile. Of the survivors, roughly three-quarters found new homes in Prussia, where King Frederick William I established them around Gumbinnen (Gusev) and where they and their descendants

remained for over two centuries until driven out by the Red Army in 1945. Other refugees went to Transylvania, the Netherlands, and south German Protestant states. A few score individuals joined Oglethorpe and the Wesleys in Georgia, where their piety, craftsmanship, and industriousness made them welcome.

In the early nineteenth century, long after the death of Leopold Anton and after the deposition of the last prince-archbishop (that Hieronymus Colloredo who mistreated Mozart and also firmly kept Protestants out of the land), and after the incorporation of the secularized state into Austria, south German Protestants settled in Salzburg anew. The revived community flourished modestly, concentrated on education, and survived the collapse of the Hapsburg Empire, the Nazis, and American occupation. Today the Lutherans of *Bundesland* Salzburg again number some twenty thousand souls.

So far, so good. This is the story—from the days of Luther, who encouraged the Salzburg Protestants, to the present—that Florey, a pastor of the Evangelical Church of the Augsburg and Helvetic Confession in Salzburg, tells in straightforward if somewhat filio pietistic fashion, with a heavy emphasis (142 out of 246 pages) on the exodus of 1731–32. The book includes a six-page bibliography, mainly of printed sources (although it is clear that the author has done a great deal of archival work), contemporary documents in quaint German, and eight pages of baroque-flavored illustrations. Indeed, Pastor Florey has been researching the history of his co-religionists for over half a century and has published four other books and three articles on the subject.

But there is little new here for the student of the various fields across which Florey's subject cuts. He did it all a decade ago in *Bischöfe Ketzer Emigranten* (1967), which is chapter for chapter and paragraph for paragraph very nearly the same book as the one under review. The new recension is slightly condensed and some errors have been corrected.

Although the present work is a fine tribute to the tenacity and adaptability of the Salzburg Lutherans—and a contribution to the history of the Empire, Protestantism, the Counter Reformation, and the early days of Georgia—it is, essentially, a hagiographical exercise, a chronicle of a chosen people. The author, his attention fixed on the virtues of his spiritual forebears, touches only tangentially on such matters as the motivations of Firmian and Cristani di Rallo, the place of the Protestants in the social and economic structure of the state, and reactions to the expulsion in Salzburg and the world beyond. One longs for a few basic tables and graphs, even for a simple map of

Salzburg and the principal areas of Protestant habitation.

There are also surprising omissions from the bibliography. Although Florey tells us about Friedrich Carl von Schönborn, the enlightened bishop of Bamberg and Würzburg, who in 1732 announced that the Salzburgers might pass unhindered through his territories and then unaccountably closed the frontier, he does not mention H. Hantsch's 1929 biography of the bishop. The 1966 SPCK publication of the letterbooks of the New England-born secretary of the Society, Henry Newman, who from London helped Oglethorpe recruit Salzburgers and other Germans for the Georgia enterprise, also appears to be unknown to Florey.

In sum, Florey's book is an old-fashioned narrative history of a persecuted church, written from the inside. Even with its shortcomings, it is a valuable record of the perdurability of one small Protestant group and of its impact on parts of the world far from its native mountains.

For those who like to see bizarre juxtapositions and odd prefigurations in history, there is this: the Salzburgers were joined in their trek by several hundred Protestants from the domains of the prince-abbot of Berchtesgaden, a place forever associated with that twentieth-century dictator who wrote terrible new chapters in the annals of the persecution of Protestants—to say nothing of Catholics, Orthodox, and Jews. One deeply feels the difference between the eighteenth- and twentieth-century varieties of persecution when one realizes that a Nazi extermination center could have liquidated the entire Salzburg Protestant problem in an afternoon and left no one alive to tell the tale.

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PETER HERSCHE. *Der Spätjansenismus in Österreich*. (Veröffentlichungen der Kommission für Geschichte Österreichs, volume 7; Schriften des DDr. Franz Josef Mayer-Gunthof-Fonds, number 11.) Vienna: Verlag der Österreichischen Akademie der Wissenschaften, 1977. Pp. x, 451. DM 80.

This major contribution to Josephinist studies argues convincingly that Late Jansenism, a political as well as theological movement, springing from France, the Low Countries, and Italy, won decisive influence in the Habsburg Monarchy between 1755 and 1790. It became the theological basis of Austrian ecclesiastical reform, i.e., Josephinism. Its goals were to reconcile religion and reason, to realize a practical Christianity, and to enlighten the people. Impeding this program were not only

the Roman Curia, the Jesuits, and their local allies, but also the mutually exclusive claims of religion and reason, church and state.

Peter Hersche's "Jansenist" is an anti-Molinist and Rigorist, who stressed Holy Scripture and the Fathers of the Church and advocated a strictly disciplined life. He is also recognized by his support for Jansenist literature, his rejection of the papal bulls condemning Jansenism, and his defense of the orthodoxy of the schismatic Church of Utrecht. These criteria identify members of the movement, mainly clerics, whose enthusiasm, good connections in the government and personal encouragement from Maria Theresa compensated for their numerical deficiency. When the movement peaked during the first few years of Joseph II's reign, de Terme was its unofficial leader, Witola its chief publicist, and Blarer its charismatic prophet. The decline of Late Jansenism was precipitous and complete. Unsympathetic to its deeper religious aspirations and alienated by the insubordination of Blarer and other radicals, Joseph merely used the movement to extend his power over the Church. Moreover, it fell into disunity concerning how much it should compromise with the Enlightenment. In the 1790s its last important representatives, such as Kaspar Karl, lived ignored and intellectually isolated.

Scorning pseudo-objectivity as well as confessional partisanship, Hersche adopts an implicitly historicist perspective. The struggles of Late Jansenists gain thereby a heightened, even tragic significance. Their well-intentioned efforts were in vain, according to the author, since ultimately if unconsciously they were contrary to the "dialectics of the Enlightenment," a radical antinomy between religion and reason. Hersche's dialectical analysis seems forced, his Enlightenment virtually reified and excessively elastic. His assertion that French and Austrian Jansenists differed fundamentally in their attitude toward the absolutist state also invites correction. They judged governments pragmatically. Anticlerical principles of church order, however, inclined them toward a gradational system of authority rather than toward absolutism. This seems implied in de Terme's objection to Joseph's militaristic model for reforming the Church.

Hersche is the first historian of Josephinism to systematically mine the rich Jansenist collection at the Utrecht Rijksarchief. He has also consulted the other important Western collections and has read critically a large body of published Jansenist literature as well as modern commentary. Although usually very accurate, he wrongly holds Bettini responsible (p. 178) for reporting to Dupac de Bellegarde that the emperor had rebuked Bishop Hay's "scandalous" pastoral letter on toleration.

Dupac's source was Wenzel Schanza's letter, March 26, 1782. The most troublesome of the few mechanical errors is a confusion of footnotes on p. 159 concerning Franz Stephan and Jansenism in Lorraine. No. 267 should refer to Wandruszka's book and no. 268 to Tavenaux's. Hersche's style is clear and vigorous, distinguished by provocative yet well grounded generalizations and brief illuminating portraits. His book should appeal to a much wider range of readers than its title suggests.

CHARLES O'BRIEN
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F. L. CARSTEN. *Fascist Movements in Austria: From Schönerer to Hitler*. (Sage Studies in 20th Century History, number 7.) Beverly Hills: Sage Publications. 1977. Pp. 356. Cloth \$17.50, paper \$7.50.

For many years Austria took the back seat in investigations into the origins of National Socialism. With a few notable exceptions such as the seminal books by Andrew Whiteside, Austrian Fascism and Nazism was regarded as a minor sideshow of little or no importance when compared to the Hitler phenomenon in Germany. Recently the inadequacy of this approach has become increasingly apparent as new data strengthens the impression that the radical Right found a congenial environment in Austria both before and after 1918. As various archival collections are opened up to scholars, a great wealth of data makes it possible for the first time to begin to produce detailed historical studies of a specifically Austrian form of Nazism.

F. L. Carsten has written a comprehensive survey of the several fascist movements that plagued Austria during the interwar decades. Avoiding almost entirely the tiresome and unenlightening theoretical discussion of precisely what "fascism" is, he has chosen to plunge directly into events. A novel aspect of the author's research is his reliance on contemporary police reports from the Austrian provinces. These reports not only provide great amounts of new information but also are a kind of window into the minds of a conservative bureaucracy often baffled by the new phenomenon of radical rightist militancy directed against both organized socialism and the bourgeois verities of the nineteenth century. Whether consciously or not, Carsten's reliance on police reports and other internal government documents has created a bias in his work that in general makes his narrative move rather choppy from event to event without any connecting thematic thread. One suspects that this is how the conservative politicians of pre-1933 Austria dealt with the emerging Nazi and *Heimwehr* movements, furiously improvising in a general atmosphere of administrative *Schlamperei*, rather

than attempting to create new political, ideological, or moral alternatives to militant nihilism.

Making little or no attempt to draw upon new sources in his opening chapters on pre-1918 ideas and movements of the Austrian extreme Right, Carsten is nevertheless able to provide a sharply-etched picture of the general milieu of racism, national militancy, and social resentments that festered barely beneath the surface of the dying Habsburg state. The defeat of 1918 and the boundless economic misery that accompanied the birth of the (unwelcomed) Austrian Republic made it inevitable that several groups basing their programs on a radical rejection of the Western, liberal-humanitarian ethos would spring to life. The book provides us with considerable detail on these early years of organizations whose *raison d'être* was political violence; what is generally missing here is an attempt to flesh out the personalities of some of these groups' leaders. The early Austrian Nazis like Dr. Walter Riehl are certainly more interesting and complex figures than are presented here. While leaving out some of the drama and color of these events, Carsten correctly emphasizes the underlying organizational problem of all these *Bewegungen*, namely their inability to agree on a united leadership and ideology. Floundering about in their own brutal rhetoric and wasting much of their energies in internal bickering, none of these groupings presented a threat to the status quo before 1927.

The Great Depression gave the Austrian Nazis and fascists their great opportunity. The fascist *Heimwehr*, led by provincial bumbler, failed on several occasions between 1929 and 1931 to seize power. Starting in 1932 Austrian Nazism and the new growth of Austro-Fascism associated with the leadership of Engelbert Dollfuss became locked in a race for supreme political power. Both Dollfuss and the Nazis desired the demise of the Social Democrats and democratic institutions in general, but the conservative and pious Catholic Dollfuss wished to retain the sovereignty of the Austrian state and at least some of the humanitarian norms of a *Rechtsstaat*. The Nazis, on the other hand, were frank irrationalists and gloried in the unleashing of destructive impulses, particularly by a thoroughly alienated youth. Carsten's narrative provides immense detail on these troubled years, but by justifiably placing an emphasis on the spectacular growth of Austrian Nazism, he neglects the clearly symbiotic relationship between the regimes of Hitler and Dollfuss-Schuschnigg. The latter governments were, after all, allowed to exist under the benevolent protection of Mussolini, and they attempted, albeit ineptly and unsuccessfully, to achieve some of the dynamic qualities of fascist Italy and the Third Reich.

In sum, Carsten has presented us with a major book that should serve as a solid foundation for other, more detailed and methodologically subtle monographic treatments of Fascism and Nazism in Austria. In the next years, other scholars will no doubt add even more to our knowledge of the *Heimwehr*, the provincial groupings, the radicalized students, and other components of the anti-democratic front. Still to be investigated is the complex relationship between Austrian Nazism and the various Sudeten-German *völkisch* groups after 1918. There is little question that Carsten's book has cleared a good deal of the undergrowth in this important but difficult area, and it should stand as a pioneering work for those who follow him into what is now no longer a wilderness.

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CHARLES L. STINGER. *Humanism and the Church Fathers: Ambrogio Traversari (1386-1439) and Christian Antiquity in the Italian Renaissance*. Albany: State University of New York Press. 1977. Pp. xvii, 328. \$30.00.

It has been more than a hundred years since Burckhardt wrote that Italian humanism was fundamentally secular, rationalistic, and pagan to the point of atheism. But it has been only within the last three decades or so that the Burckhardian position has come to be first questioned, then systematically challenged, and by now completely reversed. Beginning with Kristeller, and continued by Becker, Trexler, and Weinstein, among others, this revisionist impulse has produced a succession of pathfinding studies, perhaps the most notable of which, certainly the most monumental, is Charles Trinkaus' two-volume work, *In Our Image and Likeness: Humanity and Divinity in Italian Renaissance Thought* (1970). But while Trinkaus quite convincingly shows how the humanists felt authorized to undertake biblical criticism and religious counselling, to discuss the sacraments and monasticism, and thus generally to engage in the realm of religion—developing thereby what he calls a theological anthropology regarding man and an anthropological theology regarding God—he consciously and specifically excludes the place and importance of Greek patristic studies and of their chief exponent, Ambrogio Traversari.

That gap is now effectively filled by Charles Stinger, whose book ably describes "the religious and intellectual context of Traversari's study of the Fathers, the aim and method of his patristic scholarship, and the efforts [Traversari] made to apply his knowledge of patristic literature and of the early history of Christianity to the pressing spiri-

tual and institutional problems of the fifteenth-century Church" (p. xii). Relying heavily on Traversari's extensive correspondence, which he quotes from and paraphrases to good effect, and over which he has complete and detailed control, Stinger takes his reader through the various stages of Traversari's development. The fact that Traversari did not attend a university precluded his acquiring the panoply of late medieval scholastic assumptions, and rendered him more open to the humanistic discussions that took place in at least two of Florence's monasteries. But the fact that he entered a Camaldulensian monastery decisively influenced his development of an outlook that was at once hierocratic and contemptuous of the world (and thus thoroughly medieval). Traversari thus came to embody a unique blend of ascetic piety and humanistic intellect, a combination of classicism and pietism, or of humanism and Augustinianism, which in time served to make the Camaldolese a powerful authority in both secular and ecclesiastical camps. Stinger shows him amidst the dominant *quattrocento* controversies—whether study of the classics led to impiety, how the classics related to Scripture and the Fathers, how appropriate they were for the education of youth—following in the steps of Salutati, who wrote that "the *studia humanitatis* and the *studia divinitatis* are so interconnected that true and complete knowledge of the one cannot be had without the other" (p. 12).

In chapter two Stinger notes that Traversari tried to reconcile the ethical implications of the *studia humanitatis* with Christian piety and the monastic spirit—but it is only a passing treatment (p. 27) of a profoundly difficult problem. Was classical *hubris* or heroic *virtus* reconcilable with Christian humility, self-transcendence, charity, and hostility to pride? We are not told, though it lies at the heart of any effort to reconcile paganism and Christianity, Athens and Jerusalem. On the other hand, we are told of Traversari's extensive relationships with humanistic circles in Florence, Venice, and Rome; his travels; his exchanges of manuscripts, texts, and translations; his important relationship with the Medici. By the early 1430s the Camaldolese had entered the foremost rank of Italian humanism, comparable in stature even with Leonardo Bruni. Yet his concerns were ever with making available to the laity Old Testament texts, saints' lives, confessional and counselling tracts; with using patristic thought in humanist schools; with Church union and monastic reform.

Chapters three and four deal respectively with the renaissance of patristic studies and with the relationship of patristics to reform. In both areas Traversari sought to make accessible to the Latin West what Greek "Philosophers of Christ" had

elucidated regarding the nature of Christian faith and virtue. Stinger reviews the place in medieval thought of Origen, Chrysostom, and Augustine; he summarizes the place of patristic theology in the late Middle Ages, and shows the difference in approach between Aristotelian logic and philosophy, on the one hand, as this informed scholasticism and, on the other, patristic emphasis on the exegesis of Scripture—a distinction which in its full development I found one of the most interesting parts of the book. Indeed, the distinctive feature of Traversari's approach was that it was grounded not in Thomistic metaphysics or Aristotelian epistemology, but in the eloquence and (Stoic) ethics of antiquity—an approach that led Traversari to his proto-Erasmian view that not grace alone, but learning and grace were essential to the *quattrocento* Christian and the *quattrocento* Church. Just as Traversari understood individual Christian *virtus* to be infused with the values of ancient *pietas*, so also did he seek to reform the institutional Church along lines provided by antiquity, especially by the ancient Church, the Fathers, and the ecumenical councils which together constituted his model. But, asks the author, was Traversari too past-oriented, too hierarchical in his thinking, too ascetic in his life-style to be able to engage the urban, communal, civic Christianity of the *quattrocento*? Even Traversari himself may have wondered as he sought with few results to enlist the support of Pope Eugenius IV in meaningful reform. Stinger makes that the Camaldolese eventually discovered the limitations of eloquence alone in the struggle to change human hearts—a discovery that led him away from the activity of reform toward the more contemplative withdrawal of scholarship. But his retirement was not to be; Traversari soon found himself deeply involved in the conflict between the conciliarists and the pope, whose nuncio he became in 1435. In 1435–36 he visited the emperor in an effort to gain his support for the pope, and in 1438–39 he found himself allied with both Greeks and Latins in the effort to unite Eastern and Western Christendom. Here (chapter five) Stinger gives his reader a review of the schism, a survey of papal and conciliar politics, and an insight into the intellectual ramifications of the council, especially the semantics of theological differentiation. But union, like reform, failed to be effected in the long run, although Traversari died (October 21, 1439) before that failure was made fully manifest. By the end of the book the stage is set for the much later efforts at reform by Erasmus, the Protestants, and the Tridentine Catholics.

Altogether Stinger's is a useful, solid book, which bears witness to a thorough grasp of the sources and a good, if not unflawed, command of

detail. His assertion that "virtue is the classical *virtù* or *aretê*, the strength or energy of spirit which is identifiable also with Christian love" (p. 59) is mistaken, or at least misleading without further elaboration. Finally, I would have liked a somewhat fuller discussion of Traversari's links with the reformers who dominated the sixteenth century, and of his students, some of whom tend to be glossed over. But these are minor flaws; they do not detract from the fact that the author has capably recaptured the texture of the Christian-classical ambience within which Traversari moved and worked.

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RICHARD TILDEN RAPP. *Industry and Economic Decline in Seventeenth-Century Venice*. (Harvard Historical Monographs, number 69.) Cambridge, Mass.: Harvard University Press. 1976. Pp. ix, 195. \$16.50.

Richard Rapp's book presents a significant revision of the accepted notion of Venice's economic decline in the seventeenth century. Rapp concedes that Venice experienced relative economic decline—synchronically she yielded her position as an international entrepôt to England and Holland. But he argues persuasively that Venice did not undergo an absolute decline. By employing economic theory and statistical methods on a vast body of data culled from Venetian archives, Rapp demonstrates that diachronically population (labor force) and income (standard of living) generally remained stable throughout the seventeenth century. Therefore, he concludes, "in the purest economic sense, Venice did not decline. If income and population levels are constant, a sustained decline in output cannot have occurred" (p. 167).

Venice's decline as a center of international trade, however, necessitated adjustments in her economy. The author traces these adjustments statistically. He finds that the emigration of skilled workers from Venice was balanced by immigration of unskilled labor from the Veneto. The shift of labor away from the crippled manufacturing sector to certain service professions and the increased output of those professions compensated for the decline in employment and output in manufacturing.

In discussing adjustments in terms of technology, capital, and costs, Rapp points out that the Venetian guilds, contrary to the accepted viewpoint, were not obstacles to change; rather, guilds supported new projects and even proposed wage and price cuts. It was the conservative government, Rapp declares, which was the obstacle to

change. Its "unwillingness to countenance purposeful relaxation of standards for cost economy . . . lay at the root of Venetian competitive failures in a mercantilist world" (p. 116). Rapp's assessment here is questionable. Whereas elsewhere he has found Venetian economic behavior to be rational, he suddenly finds irrationality ("misjudgment" [p. 155]) in the government's activities. He labels the government's position conservative, but he does not give us examples of the reasoning of governmental agents in regulating the economy. In contrast to the irrationality of the ruling class as regulators, Rapp otherwise depicts nobles as careful and rational investors and businessmen.

It is clear that more work remains to be done on the dynamics of the Venetian economy in the *seicento*, especially on the activities of individuals, firms, and guilds. Students in the field may also wish to concentrate on the question of economic rationality. But it is equally clear that future research will be indebted to Rapp's seminal work.

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DANIEL M. KLANG. *Tax Reform in Eighteenth-Century Lombardy*. (East European Monographs, number 27.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1977. Pp. 110. \$10.00.

This slim monograph on northern Italy is published in a series on Eastern Europe, presumably on the argument that Lombardy was part of the Habsburg monarchy. Daniel M. Klang has given us an account of an important episode in the Italian Enlightenment, which unfortunately is too frequently overlooked on this side of the Atlantic, the tax reform in the Austrian province of Lombardy which was based on a completely new cadastre.

Essentially the reform falls into three segments. The earliest phase was the preliminary work on the *Censimento* undertaken by the *Giunta* of the Neapolitan, Vincenzo Miro, and which lasted from 1719 until the War of the Polish Succession put an end to it in 1733. Its achievement was to abolish the distinctions between the various classes of landowners, an action tantamount to doing away with privilege. In addition there was a move toward a more equitable base for assessment. The work was taken up in 1749 by the Tuscan, Pompeo Neri, who set up new local councils for the purpose of implementing and levying the taxes. In so doing he raised the annual revenue for the Vienna government to about six million lire, without impairing the further expansion of agriculture and the profits of the landowners. In the process the old nobility of birth and privilege was merely replaced by a new patriciate of affluence and culture. After Neri

was forced out in 1758, his work was taken up by Gianrinaldo Carli at the head of the Supreme Economic Council. Klang pictures Carli as a conservative who, buttressed by somewhat vague physiocratic arguments, upheld the economic claims of the larger landholders over the peasantry while politically supporting a strong absolutist regime. Throughout the book the author appears to be at pains to show us that the Austrian administration was not wholly isolated from the population in its attempt at reform, but always found some elements to support it, in spite of the opposition of the old Milanese patriciate.

It is regrettable that Klang was unable to gain access to the archival records, especially for the Neri administration, which would probably have shed some new light on the problem. As it is, he was forced to tread the well-worn paths of Italian historiography. In his eagerness for detail and direct quotations the author frequently obscures his own reasoning and makes the work difficult to follow. But the major criticism must be reserved for the shoddy editorial work, which is an affront to both author and readers. There is almost no page where there is not at least one, more often several, printing errors. One final regret is that Klang has not seen fit to set the work in its broader perspective. Apart from giving us Neri's and Carli's background, he virtually passes over the Austrian setting or the place of the *Censimento* in eighteenth-century Italian history. Yet withal the monograph is a welcome addition to the small, albeit happily growing number of English-language works on the Italian Enlightenment.

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MASSIMO LIVI-BACCI. *A History of Italian Fertility during the Last Two Centuries*. Princeton: Princeton University Press. 1977. Pp. xxii, 311. \$16.50.

Massimo Livi-Bacci's comprehensive, well-written, and well-organized volume is the third in the series produced by the European Fertility Project of the Princeton Office of Population Research. This study neatly integrates analysis of findings and suggested explanations of these findings. Livi-Bacci's central source is the Italian census from 1861 to 1971, but he uses other sources, both aggregate and micro-level, for fertility data which lengthen the time span covered and allow the examination of differentials among social groups. Livi-Bacci is willing to examine partial evidence about factors that extend beyond conventional demographic analysis; for example, he writes comfortably about cultural dimensions of fertility behavior. Livi-Bacci's personal intellectual orienta-

tion and his familiarity with pre-unification, pre-census materials and with contextual secondary research are important here.

The study is organized as follows. Chapter one examines the demography of Italy from Napoleonic times to national unification. Livi-Bacci shows that fertility, although not deliberately controlled, was well below the biological maximum, and that there were substantial differentials in fertility among some well-studied social groups. Chapter two uses the national census to reconstruct regional fertility patterns from 1861 to 1971. Here Livi-Bacci finds substantial variation, especially in the timing of the very gradual fertility decline. Chapter three compares urban and rural areas, and shows that north of Rome, urban areas had both lower fertility and higher nuptiality than rural areas. Chapter four examines the geography of fertility and nuptiality changes. Livi-Bacci notes the very real limitations of geographic analysis of fertility but nevertheless points to distinctive patterns in the South. Not only is decline late, but "territorial variability is relatively unimportant." Livi-Bacci attributes this to a "cultural unity of the South" (p. 187). The multivariate analysis of the relationships between fertility and several socioeconomic characteristics, by provinces, which he describes in chapter five, does not produce strong correlations. In chapter six, Livi-Bacci skillfully uses what data are available on family fertility behavior. He finds that the social group in which families had larger numbers of resident children shifted in the nineteenth century from rich to poor because of changes in mortality and fertility. In chapter seven, Livi-Bacci examines the possible effects of biological factors, family structure and inheritance, literacy, migration, and fascist pronatalist policy on fertility.

He concludes that "further research at the aggregate level is not likely to add to the general picture of fertility decline given . . ." (p. 289). He advocates two alternative strategies: microstudies of specific social groups based on nominative records and examination of cultural "mentalités" which must, he believes, provide the context of changing attitudes about procreation. Livi-Bacci sees a strong cultural factor at work in the fact that the South, as compared to North and Center, has relatively homogeneous fertility behavior. It is not yet evident whether this cultural factor will look as salient once individual-level local studies are done which can examine more subtle and complex indicators of economic and social variables. Livi-Bacci has given us an indispensable base-line for all future research.

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GUIDO DONNINI. *Il 1917 di Russia nella stampa italiana*. (Istituto di Storia Moderna e Contemporanea, number 6.) Milan: A. Giuffrè. 1976. Pp. 432. L. 9,000.

For Italians, as for all other Europeans, the year 1917 was extremely turbulent. As the war continued with no end in sight, the social, psychological, and economic costs intensified the antagonism between the government and the people. To add to the confusion and turmoil within Italy, there was, throughout the year, interest and concern with the developments in Russia.

An investigation of that interest and concern, through an examination of various newspapers and journals, is the focus of Guido Donnini's book. The author, however, admits that it is difficult to know whether public opinion determines what appears in print or whether newspapers and journals form public opinion. Hence he makes no judgments about the reaction of Italian public opinion to the various events in Russia in 1917; he simply allows the press to speak for itself.

The book is valuable as a collection of articles from some of the major newspapers and a few respected journals. There are long excerpts from *Corriere della Sera*, *La Stampa*, *Avanti!*, *Il Popolo d'Italia*, and *Il Giornale d'Italia*, as well as from journals such as *Critica sociale* and *La Nuova Antologia*. The author, however, fails to analyze, beyond a few superficial comments, the contents of the articles or the historical context in which they appeared. He plods through each of the five chapters with a grim determination to include every article from every source. The repetition that results from this "scissors and paste" approach proves tedious. The book could have been more analytical and less repetitious had the author organized his chapters so as to examine the chief ideological or political responses to the February Revolution, the Kornilov Affair, or the Bolshevik seizure of power.

The articles, however, provide important information on the various ideological strains in Italy in 1917. They show Mussolini and his *Popolo d'Italia* virulent in their anti-Bolshevism and in their fear that the Russians would sign a separate peace. Luigi Albertini and his *Corriere della Sera* appear more sober than Mussolini in their antisocialism, but just as concerned about the possible effects of the events in Russia on the Allied war effort. The socialists, in *Avanti!*, and Gaetano Salvemini, in *L'Unità*, seem hopelessly tangled in their ideological confusion until, finally, in order to explain how the revolution could have occurred in such a backward society, they assert that the Bolsheviks are not Marxists. Not the most sophisticated analysis, but, as the articles in the book demonstrate, the

socialist press tended to approach the news more analytically than the others. Both *Corriere della Sera* and *La Stampa*, for example, ordinarily restricted themselves to a factual account of events, and the best analyses to appear in the latter generally came from articles translated from the English and French press.

By the end of the year none of the sources consulted by Donnini approved of the Bolshevik Revolution, but only one, the reactionary, anti-Semitic *Il Giornale d'Italia*, had already begun the appeal for an Allied intervention to crush the new regime. Even Mussolini shied away from intervention, since he still ranked victory over Germany and Austria-Hungary as the most important goal for all Italians. The excerpts in this book cover much more, but the history of the Italian press and its reaction to 1917 in Russia remains to be written.

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MARTIN CLARK. *Antonio Gramsci and the Revolution that Failed*. New Haven, Conn.: Yale University Press. 1977. Pp. x, 255. \$15.00.

This book has two principal subjects, as Martin Clark indicates in his preface. They are the Turin labor movement from about 1914 through 1920, and the political career of Antonio Gramsci, especially as it related to that movement. Major emphasis, particularly in the more deeply researched chapters, is on the Turin workers, their creation of factory councils, and the extremely dramatic "occupation" of the factories in September 1920.

It is a remarkably solid, well researched, and convincing narrative. Clark's employment of material from the National State Archives at Rome and his judicious use of most of the voluminous published material of recent years is truly impressive. His will remain an indispensable work on the subject for a long time. Those readers with a special interest in factory councils and their development in this crucial period can concentrate on chapter four and chapters six through ten. But they should not neglect the introduction, which presents Clark's theory of workers' control over production. One of his basic points is that the council movement "relied mainly on skilled engineering workers—the labor aristocracy of the time—and had little appeal for others" (p. 7). This is an important idea, but it is controversial and not completely convincing if we confine the discussion to the attitudes of "skilled" and relatively "unskilled" workers in the Turin "metallurgical" plants. On the other hand, Clark is absolutely correct in rejecting the negative attitude toward councils of many labor leaders and scholars who claim that "workers

'participation' would mean that workers would acquire managerial views." As Clark says, this is the "whole point." In Gramscian terms, the movement of the working class from a "subaltern" to a "hegemonic" position would be impossible short of mastering the process of production.

Why then does Clark assert that "Gramsci made his greatest mistake" in welcoming the Taylorist "Scientific Management of Labour" in the name of productivity" (p. 9)? It is true that "Taylorist schemes . . . dealt a mortal blow to the industrial power of skilled craftsmen," but such things happened under the domination of ownership, which was not what Gramsci had in mind. Here Clark might have resorted to Lenin, that other Taylorist revolutionary of the time. In April 1918, the Russian leader declared that the "possibility of building socialism will be determined precisely by our success in combining the Soviet government . . . with the modern achievements of capitalism. We must organize in Russia the study and teaching of the Taylor system and systematically try it out and adapt it to our purposes." Gramsci probably never read this speech, but it certainly bolsters his view of the councils. In all other respects, Clark's discussion of the "productivist" element in the council movement is very good.

Another strong point in this book is Clark's assessment of Gramsci's views on the role of the political party in the revolutionary process: "Gramsci never minimized the importance of the Party's role . . . and urged that the Councils could not hope to succeed unless they were strongly influenced by a militant Party" (p. 62). In this, he lays to rest the charges of Bordiga and the Maximalists that Gramsci was tainted by "anarcho-syndicalism" in 1919–21. The peculiar isolation of Gramsci from practically everyone else on the Left in late 1920 is also demonstrated more effectively than ever before (chapters six and seven). Finally, Clark's brief summary of the importance of Gramsci's *Ordine nuovo* movement to the future of the Italian Communist Party (pp. 212 and 218) is right on target.

My only serious criticism of this work is that Clark occasionally exaggerates the certainty of his conclusions. One example is his evaluation of Gramsci's "interventionism" in the summer of 1914 (i.e., his alleged support for Italy's entry into World War I). As Clark points out, the present reviewer in an earlier work unduly minimized Gramsci's stand for intervention. He himself goes much too far in the other direction when he states that "Gramsci was 'interventionist'" (p. 49). It is simply impossible to be that definitive about Gramsci's obscure and tortuous public statement of October 31, 1914. The best proofs of this are the extremely cautious discussions of the question in

Giuseppe Fiori (*Antonio Gramsci: Life of a Revolutionary*, pp. 96–98) and Paolo Spriano (*Torino operaia nella grande guerra*, pp. 86–88). In any case, Clark is completely unjustified when he claims that the *Bolshevik Revolution* led Gramsci “to turn away from his quasi-Salvemian ‘interventionist’ views . . .” (p. 52). As Angelo Tasca wrote on this point, Gramsci, unlike Palmiro Togliatti, “had begun his own ‘self-criticism’ very quickly and very seriously” (*I primi dieci anni del PCI*, p. 96).

Finally, Clark’s conclusion and postscript perhaps attempt to resolve too many problems in too few pages. There are indeed many acute observations, for example on the “legacy of the *Ordine nuovo*”; however, there are also a fair number of rather sweeping generalizations. Among these is the claim that Togliatti, while preserving the Gramscian heritage for the PCI, also “renounced Gramsci’s ‘revolutionary’ perspective, and . . . pursued a respectable ‘anti-fascist’ line that typified the ‘third weakness’ [i.e., “reformism,” social democracy, etc.] discussed earlier in this Conclusion” (p. 225). Togliatti certainly did renounce, if that is the word, Gramsci’s *specific* revolutionary perspective, but it is rather too simple to dismiss the “*Partito nuovo*” and “*la via italiana al socialismo*” as mere “reformism.”

Similarly, Clark maintains that the “right-wing” Marabini-Graziadei Communist faction of 1921 established “a tradition that has survived to this day in Emilia, especially in the Bologna area” (p. 206). My recent research in the National State Archives on the Emilian sections of the P.C. d’I. indicates that they already had their full quota of “Leftists” (i.e., *Bordighisti*) in 1925–26. More important, the Bolognese Communists have firmly controlled their “fat, learned, and red” city for thirty years and have therefore been continuously involved in the responsibilities of daily administration rather than more traditional and marginal “militant” activities. How can a unique situation such as this be contained within the framework of the usual “right-left” analysis?

Despite these criticisms, I consider this a very fine book, a pleasure to read, and indispensable to all students of the Italian labor movement and the formative years of Italian Communism.

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CAROLE ROGEL. *The Slovenes and Yugoslavism, 1890–1914*. (East European Monographs, number 24.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, N.Y. 1977. Pp. vii, 167. \$12.00.

Carole Rogel here discusses political and cultural Yugoslavism as manifested between 1890 and 1914 among Slovene political leaders and intellectuals. The political aspect of Yugoslavism involved programs which sought either to create a South Slavic state inside the Habsburg empire (such as the trialistic solution or the transformation of Austria-Hungary into a federation of nations) or to unite the Habsburg South Slavs with the kingdom of Serbia outside the old empire. Cultural Yugoslavism encompassed among other ideas the neo-Illyrian plan to abandon the Slovene language for Serbo-Croat, or more moderate solutions, such as merely accepting into the Slovene language merely new scientific terminology common to all the South Slavs, or, at the other extreme, advocating cultural exchanges among the South Slavic peoples only to learn of each other’s language, literature, and arts.

The Slovenes and Yugoslavism, 1890–1914 is divided into nine chapters. The first two give a historical background emphasizing the development of a Slovene national consciousness and the formation of the first political programs. Each of the next three chapters describes the organization of one of the three Slovene political parties—the Catholic People’s Party, the liberal National Progressive Party, and the Social Democratic Party—and explains each party’s position toward a Yugoslav program prior to the Bosnian crisis of 1908. Chapters six and seven deal with the 1908–09 crisis and its impact on political and cultural Yugoslavism. The revival of Illyrian ideas of the 1830s and 1840s is given particular attention.

Chapters eight and nine describe the Balkan wars of 1912–13 which undermined faith in Austria-Hungary and gave rise to the political program advocating a South Slavic solution outside the Habsburg empire. This first true Yugoslav program was propagated by individual intellectuals and the *Preporod*, an organization of upper gymnasium students. Prior to 1914 no Slovene political party accepted this Yugoslav solution outside of the Habsburg framework. Only a few radical students and still fewer intellectuals propagated it. Moreover, all the political programs searching for a solution either inside or outside the Habsburg empire were, according to Rogel, general and foggy, and had no specific statement about the position of Slovenia in such a new South Slavic state. Hence, she rightly chooses to conclude her book with some clear words on this topic spoken by Ivan Cankar, the greatest Slovene contemporary writer, in a talk in Ljubljana (April 1913) in which he advocated a Yugoslav republic outside of the Habsburg empire, if the majority of Slovene people so wished, but only if the Slovenes entered such a new federation as equal partners

and had their own self-government. Cankar favored a political union but declined outright any cultural or linguistic one. For him the Yugoslav problem was only political.

Rogel's main topic, presented in seven out of the nine chapters, is the result of thorough research. She used primary sources in Slovenia such as archives, contemporary publications, articles in newspapers, magazines, and reviews. In addition, she interviewed three surviving members of the *Preporod*. Her book is therefore an important contribution not only to Slovene history, but to the history of the Habsburg empire, of Yugoslavia, and of the Balkans.

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JOHN D. BELL. *Peasants in Power: Alexander Stamboliiski and the Bulgarian Agrarian National Union, 1899-1923*. Princeton, N.J.: Princeton University Press. 1977. Pp. xiv, 271. \$16.50.

The Bulgarian Agrarian National Union, which governed the Balkan kingdom from 1919 to 1923, has attracted considerable interest as a modern non-Communist revolutionary organization. Yet, there has been little scholarly study of the union or assessment of its activities and achievements. Even in Bulgaria, studies have been limited to a handful of articles and tangential monographs by historians such as Vladislav Topalov, Aleksandur Vele, and Dimitrina Petrova. Most full-scale "histories" of the union and its leaders are better classified as memoirs or polemics. In part, the reasons for this gap in historical literature are the relatively recent creation of the union, its political sensitivity in some areas, and the destruction of most of its records in the turbulent years 1923-25. Under these circumstances John D. Bell's pioneering study of the rise of the union and the years of its government is most welcome. Bell not only uses the standard extant sources—the union's press and extensive memoirs—but also was one of the first Western scholars to gain access, albeit limited, to Bulgaria's Central State Historical Archives. In addition, he makes use of English and French diplomatic reports and the private archives of Dimitur Stanchev, King Ferdinand's personal secretary, which are located in this country.

Bell's monograph is an extension of his dissertation, which ended at the point of the BANU's coming to power. His work centers on Aleksandur Stamboliiski, the union's renowned leader. Stamboliiski, while not one of the BANU's original founders, effected the change in the union from a static, pedantic, semipolitical, semieconomic organization into the most powerful political party in

Bulgaria. His charismatic leadership, astute ideological logic, and iron will forged the principles and tactics of the union as he himself became one of the most controversial statesmen of modern Europe. Bell portrays Stamboliiski not only as a dynamic and capable political organizer, but also as a creative ruler and bold diplomat. He compares him to Lenin in his attempts to provide for an exhausted and unjust society the basis of a modern egalitarian state (p. 183). Individual historians may wish to argue with Bell on some points, e.g. how much Stamboliiski's peasant ideology was a thorough and sound political philosophy or how much Stamboliiski can be excused from the excesses against civil and political liberties committed by the BANU government; but the author's interpretations are based on sound scholarship and are legitimate assessments. This work will long stand as a valuable contribution to the history of modern Europe.

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ȘTEFAN PASCU *et al.*, editors. *Independența României* [The Independence of Romania]. Bucharest: Editura Academiei Republicii Socialiste România. 1977. Pp. 526. 39 L.

The purpose of *Independența României* is divulged on the jacket of the book. "Romania's independence was gained through the war of 1877. The securing of our national independence . . . , the result of the heroic struggle of the Romanian army against the Ottoman empire, of the firm determination of our people's quest for liberty, has exerted a profound impact on Romania's total historic evolution on the road to social progress . . ." This statement, extracted from the program of the Romanian Communist Party, is re-echoed in a supplementary quotation by President Nicolae Ceaușescu which equates independence with "the right of all peoples to unhindered mastery of their destinies on the basis of complete equality of rights, respect of national independence and sovereignty, noninterference in internal affairs and renunciation of force or threat of military action in intra-state relations." And the historic "i" is dotted with a final statement by the Central Committee of the Romanian Communist Party. "The historic . . . responsibility of the Romanian Communist Party to the Romanian people is the organization of the struggle for a free and independent Romania."

The legitimacy of the Romanian Communist state and Party and of the supreme leader of both, Nicolae Ceaușescu, is stated to be derived from the

Romanians' historic struggle for national independence and from the Communists' determination to defend this supreme national achievement. Accordingly, this volume is expressly designed to prove the validity of this contention.

The numerous contributors provide historical evidence supportive of the claims of the Romanian Communist Party and of its leader. The editor-in-chief, Ștefan Pascu, places the entire volume in the proper perspective by means of a general foreword and a well-documented introductory chapter entitled "Independence—the Fundamental Characteristic of the History of the Romanian People." The ensuing studies are variations on the same theme and include articles on the affirmation of the bravery of the Dacian fighters in defense of liberty by C. Preda; on the struggle for independence of the Romanians during the Middle Ages by Ștefan Ștefănescu; on the idea of independence during the Enlightenment by P. Teodor; on several aspects of the quest for independence during the first three quarters of the nineteenth century by such well-known historians as Constantin Giurescu, Cornelia Bodea, and Dan Berindei; and, finally, on the war of independence by Ștefan Pascu, Ilie Ceaușescu, G. Muntean, and other specialists. The post-1877 period, focusing first on the completion of national independence through the establishment of Greater Romania at the end of World War I and again, after World War II, by the Romanian Communist Party, comprises a large number of diverse studies, historic and political, prepared by professional historians such as Viorica Moisiuc and Gheorghe Zaharia, and by politically active members of the party including the Minister of Foreign Affairs, G. Macovescu. Among these the most interesting are Gheorghe Zaharia's on the defense of Romania's sovereignty and independence by the "democratic and revolutionary forces" during the interwar years—in which the author broaches several delicate problems including the loss of Bessarabia and Northern Bukovina to the Soviet Union—and the contributions of D. Mazilu, M. Voiculescu, and G. Macovescu, all concerning the party's current determination to safeguard Romania's independence. Appropriately, the concluding paragraph of the book consists of a quotation by Nicolae Ceaușescu: "We must learn the lessons of our fatherland's history and do everything to insure the continuing development of our people, of our nation, of our national independence and sovereignty . . ." (p. 522)

The studies which comprise the book are intelligent and scholarly. It is fair to say that the historic evidence presented by the various authors generally validates the overall conceptual framework. What was left unsaid is also significant. The

nationalism of the Romanian Communist Party, whose ultimate expression is the assumption of the role of completer and defender of Romania's independence, cannot be rationally equated with the actions of the Dacians or, for that matter, with the ensuing stages of the Romanians' quest for independence. There are cogent reasons for singling out the safeguarding and furthering of Romania's independence as the *raison d'être* of the Romanian Communist Party and of its leader, Nicolae Ceaușescu, in our times. The explications provided by the authors of the book are elliptical, enigmatic, and equivocal. Nevertheless, students of Romanian history and politics will find the reasons advanced for justifying the equating of the new *defensor patriae*, Nicolae Ceaușescu, with the valiant historic *voevods* of yore intriguing and at times even persuasive.

STEPHEN FISCHER-GALAȚI
University of Colorado,
Boulder

KEITH HITCHINS. *Orthodoxy and Nationality: Andrei Șaguna and the Rumanians of Transylvania, 1846–1873*. (Harvard Historical Studies, number 94.) Cambridge, Mass.: Harvard University Press. 1977. Pp. ix, 332. \$14.00.

In 1969, Keith Hitchins published *The Rumanian National Movement in Transylvania, 1780–1849*, presenting the history of the movement mentioned in the title from its inception to the end of the revolutionary events in the Habsburg monarchy. The present volume continues this survey to the year of Andrei Șaguna's death. The slight chronological overlap is justified not only by the fact that Șaguna became bishop and head of the Romanian Orthodox Church in 1846, but also by a treatment of the revolutionary years differing from that in Hitchins' first volume.

While concentrating on Bishop Șaguna, the major spokesman of the Transylvanian Romanians at least until the end of 1864, the book discusses in ample detail the development of the Transylvanian Romanians, Orthodox and Uniate, laymen and clerics, peasants and intellectuals, as well as imperial and Hungarian attitudes toward them. The author pays attention to political, cultural, and educational matters, as well as to purely ecclesiastical issues. He succeeds admirably in making this second study a continuation of the subject to which the first was devoted.

Selecting bishop Șaguna as the central figure of this period is fully justified. The Romanians of Transylvania were fortunate to have him as their bishop during the crucial years under review. Both they and Vienna considered the Orthodox and

Uniate bishops as the leaders and spokesmen of the Romanians. In Șaguna the Orthodox acquired a leader who was not only a first-rate theologian and writer, but also an activist trained in business management and law who understood their problems, aspirations, and weakness. An indefatigable worker, Șaguna was active as a political and educational leader, author, editor, teacher, and negotiator without ceasing to be, first and foremost, a bishop. Crucial to understanding him and his work is an insight into his views on Church and state relations and on the role of the Church in the life of the nation. Hitchens repeatedly stresses these issues and makes them abundantly clear.

While Șaguna was gradually supplanted by lay intellectuals as the chief spokesman of his people in the second half of the 1860s, his major achievements, the establishment of an independent Transylvanian Romanian Metropolitanate and an Orthodox school system, became the bulwarks of the Romanian national struggle directed against the steadily growing trend of Magyarization after 1867. His service to his people survived him.

Carefully researched, well documented, clearly written, and well edited, this volume lives up to Hitchens' reputation as a first-rate scholar. It is to be hoped that he will write a third volume continuing the history of the Transylvanian Romanians from Șaguna's death to the birth of Greater Romania.

PETER F. SUGAR
University of Washington

PHILIPPE MARGUERAT. *Le III^e Reich et le pétrole roumain, 1938-1940: Contribution à l'étude de la pénétration économique allemande dans les Balkans à la veille et au début de la Seconde Guerre mondiale.* (Collection de Relations Internationales, number 6.) Leiden: A. W. Sijthoff, for Institut Universitaire de Hautes Études Internationales, Geneva, 1977. Pp. 231.

The Institute of International Studies in Geneva is sponsoring research and publication of works on Nazi economic penetration in the Balkans in the 1930s. This volume surveys and attempts to analyze Romania's place in the Reich's politico-economic system, Romania's financial relations with Nazi Germany and the other major powers, general commercial relations, and the Third Reich's impact upon Romania's domestic and foreign policies. It concentrates upon the period from the summer of 1938 to September 1940 when the fascist regime of Marshal Antonescu acceded to power in Bucharest.

In the 1930s, just as in the period prior to Romania's entry into World War I in 1916, Romania's petroleum industry was a prize to be

seized by oil-hungry belligerents. Romania's oil fields were the only ones of any size to be found in Europe outside of Soviet Russia. The peak of oil production came in 1934, with 4.05 percent of total world output. By 1938 production fell to 2.41 percent of world output. The fall resulted from exhaustion of the oil deposits; moreover, exploration for new fields became difficult because of the lack of native investments and technical ability and of external assistance. It is thus manifest that the Nazi penetration of Romania's oil industry was primarily due to Berlin's strategy designed to prevent others from moving in and to attract Bucharest to the Axis. In March 1939, Romania agreed to an economic arrangement whereby her economy was almost integrated into that of Germany's. The treaty curiously stressed agricultural productivity, as if to underscore the decline in oil expectations. Romania was reduced to being a Nazi economic satellite; one and half years later Nazi forces occupied the country.

This monograph, like the treaty, has its curious features. Philippe Marguerat could not undertake research in Romanian archives, an obstacle shared by most Westerners, but he has not done his homework in the free world. Certainly he should have taken tips from the valuable analysis of Maurice Pearton's *Oil and the Romanian State* (1971) and given less heed to documents in Nazi archives. Berlin's aims in the region need no reiteration; Romania's gyrations in the 1930s need investigation and analysis. Nevertheless, Marguerat has produced a valuable analysis of Germany's methods of countering Anglo-French economic and political maneuverings in Romania, and the success of these methods in making the British and French appear to be "sleepwalking" and even treacherous after London granted assurances to Romania in April 1939.

SHERMAN D. SPECTOR
Russell Sage College

ZBIGNIEW WÓJCIK. *Rzeczpospolita wobec Turcji i Rosji, 1674-1679: Studium z dziejów polskiej polityki zagranicznej* [The Polish-Lithuanian Commonwealth in Relation to Turkey and Russia, 1674-1679: A Study from the History of Polish Foreign Policy]. Summary in English. Wrocław: Ossolineum, 1976. Pp. 249. 60 Zł.

One of the tragic ironies of the reign of Jan Sobieski is that, despite the monarch's dynamic, intelligent, and well-intentioned qualities, he expanded an excessive amount of resources and energy in warfare against the Turks—to the neglect of other pressing international and domestic issues. A striking example of the result of this is, of

course, the fact that less than a century after Sobieski helped to pull some Habsburg chestnuts out of the fire by participating in the defeat of the Turks at Vienna (1683), the Austrians responded by participating in the First Partition of Poland. In point of fact, however, despite the glory and fame that accrued to Sobieski by virtue of being a "crusader" against the Islamic infidels—which is considered, quite correctly, his major contribution to Polish, and European, history—he was not as simple-minded in this regard as most historical accounts of him seem to imply.

This conclusion is amply illustrated in the monograph under review. Sobieski recognized in the first years of his reign the desirability of concluding an alliance with the Turks so as to free the Commonwealth for other important business: internationally, to settle accounts with Brandenburg Prussia, Russia, and perhaps with the Habsburgs; domestically, Sobieski sought to establish a strong monarchy in the Commonwealth and secure the royal succession for his eldest son by acquiring a fiefdom for him in Ducal Prussia or Silesia (or, that failing, from some Turkish-held territory) to strengthen his electoral chances upon the father's death. The "French connection" was quite strong in the first years of Sobieski's reign; in fact, it was partly due to French influence that he was elected king in the first place. The pro-Turkish, anti-Habsburg policies of Louis XIV offered Sobieski some apparent opportunities; the price to be paid would be peace with the Turks.

The chance came in 1676, when a truce between the Poles and Turks was signed at Żurawno, followed by the dispatch of Jan Gniński to Istanbul for the signing of a peace treaty. Turkish intransigence, however, prevented an agreement from being reached; likewise, plans to invade East Prussia in cooperation with Sweden came to naught; and, finally, the alternative possibility of an anti-Turkish alliance with Russia also failed because of Muscovite indifference. Reluctantly, but with seeming inevitability, Sobieski and the Commonwealth moved away from France and concluded a military alliance with Austria. The upshot would come at the siege of Vienna in 1683.

As depicted by Zbigniew Wójcik, Sobieski's role on the international stage was only part of a complicated tug-of-war between several competing interests: French expansionism, Swedish ambition in the Baltic, Brandenburg intrigues, growing Russian involvement in European affairs, Habsburg and papal clamor for an anti-Turkish crusade, and the role played by Turkey. With the key position of Grand Vizier in the hands of the ambitious Köprülü family, there was a "renaissance" of Turkish power. Short-lived though this proved to be, the Turks came close to conquering Vienna

in 1683. One of the most interesting aspects of Wójcik's monograph deals with this twilight of imperial Ottoman splendor.

Wójcik, one of the outstanding Polish authorities on the subject of Poland's relations with Russia and Turkey during the second half of the seventeenth century, has produced several important books on the subject. This latest study is as clearly defined in scope and well-documented as its predecessors. The author's numerous archival sources as well as secondary works in a number of languages have been used to good advantage. The absence of Turkish-language materials, aside from several secondary works, does not seriously detract from the book's scholarly value.

HARRY E. DEMBKOWSKI
Alliance College

CATHERINE S. LEACH, editor and translator. *Memoirs of the Polish Baroque: The Writings of Jan Chryzostom Pasek, A Squire of the Commonwealth of Poland and Lithuania*. Foreword by WIKTOR WEINTRAUB. Berkeley and Los Angeles: University of California Press. 1977. Pp. lxiv, 327. \$20.00.

The *Memoirs* of the petty nobleman Jan Chryzostom Pasek (1636?–1701?) are a literary classic and an important historical source depicting Polish society amid the devastating wars and magnate intrigues that accompanied the decline of royal authority. This skillful translation is enjoyable reading, vividly capturing Pasek's colloquial style. The nonspecialist will not miss the minor excisions: two passages on the Lubomirski insurrection and some flowery verse and oratory. Catherine S. Leach's introduction and appendixes, which discuss the historical context, are well informed. The book is a fine addition to the scanty literature in English on Old Poland and should be issued in paperback.

Pasek made a modest fortune as a soldier, acquired land, and spent his later years feuding and litigating. As a description of gentry life, the *Memoirs* are rivaled only by the writings of the sixteenth-century squire Mikołaj Rej. Pasek, like Rej, married an heiress, sold grain profitably in Danzig, obtained small royal land grants, and leased neighbors' estates (a common practice that caused incessant quarrels among the nobles and misery for the peasants). Both portray Poland as a disorderly land in which survival depended on influence; both deplore courtly corruption and gentry irresponsibility. But Polish society had grown more brutal—and the magnates more powerful—during the century between Rej's lifetime and Pasek's. By the seventeenth century the gentry, having lost the political initiative and without a realis-

tic reform program, found themselves buffeted between royalist and antiroyalist magnate factions. Pasek's first loyalty was to his military commander, a member of the dominant French party. He disliked the *liberum veto* (which he saw as an instrument of magnate domination) and supported Jan Kazimierz when that king was challenged by mutinous troops and by the rebellious magnate Jerzy Lubomirski. Yet Pasek sympathized with Lubomirski's professed aim of defending the nobles' Golden Freedom. Pasek's favorite monarch was "humble as pie" Michał Wiśniowiecki—actually the puppet of the pro-Habsburg magnates whom the Francophiles plotted to dethrone. Only under such a *fainéant* could the gentry safely long for an ideal king who would keep order and suppress factions without infringing Polish liberties.

JAMES MILLER
Stockton State College

ANDRZEJ FELIKS GRABSKI. *Myśl historyczna polskiego Oświecenia* [Historical Thought of the Polish Enlightenment]. (Dzieje polskiej myśli historycznej.) Warsaw: PWN. 1976. Pp. 485. 85 Zł.

This book shows Enlightenment historical thought in Poland to have been richer and more complex than is often thought. Like other historians, Andrzej Grabski presents the monarchical school exemplified by Felix Łojko and Adam Naruszewicz. The former collected sources on economic history and wrote several unpublished works, most notably one justifying Polish claims to lands seized in the First Partition. The latter wrote a *History of the Polish Nation* (five volumes, to 1386), using many original sources. The author also presents enlightened republicans like Michał Wielhorski. But another, often overlooked, school of historiography which Grabski introduces is the "Sarmatian" school, which ran in counterpoint to the enlightened voice. Sarmatian historiography was based on the myth of the golden past and expressed itself in biographies and tracts on the origins of institutions like the *liberum veto* or the elective monarchy. While most Sarmatians argued theologically that Poland's decline came as punishment for her sins, a few developed a secularized version based on social forces. Sarmatianism was widely held and influenced Romantic nationalism.

The Four Year Diet stimulated historical reflection among parliamentarians and publicists. Grabski reports the debates in the Diet but pays special attention to the writers of Kołłątaj's *Forge*, who pressed for greater social equality, citing historical evidence to support their views. After the partitions, four schools of historical writing

emerged, according to Grabski. From right to left politically, they were the post-Sarmatians, the monarchists, the moderate reformers, and the democrats; a Slavophile school also appeared. Most of the histories written in this late Enlightenment were not successful, Grabski claims. First Jan Albertrandi, and later a variety of authors commissioned by Warsaw's Society of the Friends of Science, attempted to complete Naruszewicz's unfinished *History*, but they failed to address themselves to new questions which interested the reading public.

Two poets conclude Grabski's account. Julian Ursyn Niemcewicz wrote *Historical Songs* (and some nonfiction) in the spirit of the May 3 constitution to celebrate both the contributions of Poland's kings and the development of Polish liberties. Jan Paweł Woronicz continued the providential Sarmatian tradition of presenting Polish history as a drama of sin and redemption. Both used history to "strengthen the hearts" of Poles. Their works provided a bridge to romantic historical thought, which will be the subject of Grabski's next book.

This volume is carefully presented and extremely well documented from historical works, debates of the diet, a considerable variety of archival sources, and some literary evidence. The author also uses the impressive secondary literature on the subject.

Despite these virtues, the book suffers from a highly restrictive definition of history. For Grabski, as for his late mentor, Henryk Serejski, historiography merely represents the political views of the historian. The past cannot be approached with any objectivity and historical studies become an adjunct to the study of Polish political thought. As a result, his book is a complex and sophisticated version of the old, tiresome game of optimists and pessimists. It might be more accurately entitled, *Historical Examples Used in Polish Political Debate during the Enlightenment*.

DANIEL STONE
University of Winnipeg

TADEUSZ KISIELEWSKI. *Heroizm i kompromis: Portret zbiorowy działaczy ludowych*. Volume 1, *Okres zaborów* [Heroism and Compromise: A Composite Portrait of Populist Activists. Volume 1, The Period of the Partitions]. Warsaw: Książka i Wiedza. 1977. Pp. 359. 70 Zł.

At first glance, this modest collection of biographical sketches on various leaders of the Polish peasant movement prior to World War I appears insignificant, perhaps even superfluous. After all, the literature on this topic published in Poland is fairly

extensive, and it would appear that little new information on the early Polish populists could be forthcoming. A close reading of the book would seem to vindicate this judgment, for anyone familiar with the historical studies focusing on the Polish peasant movement will find no major factual revelations or items of detail herein.

Yet it would be a major error to dismiss this book as devoid of value. Indeed, several factors combine to make it an important publishing event in Polish historiography. For one thing, Tadeusz Kisielewski enjoys considerable political status as a leading editor of *Wież Współczesna* ("Contemporary Countryside"), the official monthly publication of the Central Executive Committee of the United Peasant Party (ZSL). This does not diminish his stature as an accomplished historian, for he has produced many superb essays on Polish populist and agrarian history. More important is the general thrust of the book. In viewing the origins and evolution of the peasant movement in the Austrian, Russian, and Prussian/German zones of partitioned Poland, the author deviates from the traditional practice of categorically castigating those leaders who tried to work within the existing system to effect change, and blindly praising those who adamantly opposed the system and struggled for its total destruction. The very title of this book—*Heroism and Compromise*—suggests a somewhat different approach. Although he stops short of openly exalting those early peasant leaders who struck bargains of expediency with other Polish political movements and organizations, Kisielewski does portray their actions realistically and with understanding and sympathy. Words of praise still exist for the more irreconcilable militants to whom any compromise was anathema, but even here a certain degree of cautious criticism of their rash, unrealistic policies surfaces. Of course, this slight change of interpretation could be misread and misrepresented to make it seem the current political structure in Poland is about to undergo a major transformation; such a conclusion is not at all what this reviewer intends. Rather, the book should be received as welcome proof of the continuing maturation of Polish historiography of the peasant movement, which hitherto has been handled quite well but has not been immune from polemical approaches.

The book is well written in a clear and smooth style, in language and format designed more for general public consumption than for a narrow circle of erudite scholars. With its biographical emphasis it complements nicely the numerous Polish-language studies of the Polish peasant movement and such English-language classics as Stefan Kieniewicz's *Emancipation of the Polish Peasantry* (1969) or Peter Brock's *Nationalism and Populism in*

Partitioned Poland (1973). It certainly deserves to be read by anyone interested in the Polish political scene prior to World War I, as well as the Polish peasant movement.

EDWARD D. WYNOT, JR.
Florida State University

CELIA S. HELLER. *On the Edge of Destruction: Jews of Poland between the Two World Wars*. New York: Columbia University Press. 1977. Pp. xi, 369. \$14.95.

As a result of the Nazi Holocaust and, most recently, because of re-emerging Russian anti-Semitism, the fate of European Jews has attracted the attention of many historians, especially in the cases of Nazi Germany and the Soviet Union, though serious studies dealing with other countries are rare. Despite their number—over three million—Jews residing within interwar Poland remain so far without a comprehensive account. The available English literature on this subject is mostly fragmentary, scattered throughout numerous periodicals, often of a popular nature.

The remedy has come from Celia S. Heller, professor of sociology at Hunter College and the Graduate Center of CUNY, born in Galicia into a Jewish family. Her work, showing her to be not only an accomplished sociologist but also a knowledgeable historian, is presented in the tradition of sociology within the framework of history. Considering the peculiarities of Jewish minority life in a hostile society, and the author's stated intention to tell the story of the Jews in Poland during the penultimate period of their existence in the shadow of modern anti-Semitism, this particular method succeeds well.

Against the historical perspective of some six centuries, Heller unfolds a panorama of definitions, forms, patterns, confrontations, struggle for survival, internal conflicts, and desperation affecting the broad strata of Jewish society. The great majority of Jews had welcomed Poland's independence and, their position secured by the Minorities Treaty of 1919 and Constitution of 1921, looked confidently to the future. However, the Jews soon faced the growing indifference of the state and a spreading anti-Semitism among the Poles. These experiences are the subject of the bulk of the study with a concentration on the pattern of oppression, use of terror and abuses, and the responses of Jewish groups and parties to the situation. In search of security the Jews developed two main patterns: the orthodox traditional and the assimilationist. Followers were divided unequally—traditionalists adhering to Judaism and Torah were the great majority, especially the lower social classes, while assimilationists, sub-

scribing to secularism and modern ideologies, remained an insignificant minority. Jews soon learned that escape from their Jewishness, their way of life, and their ethnic consciousness had failed to bring about the expected improvement. As a result, conversion to Catholicism, acculturation-polonization, and secular assimilation decreased in response to the wave of anti-Semitism affecting education, employment, and economics.

On the political level the deteriorating position of Jews made the Zionist movement popular at the expense of the socialist Bund and liberal views. The politics of despair and self-defense seemed the only possibility in a hostile environment.

The book offers a thorough study in social confrontation, minority versus majority, individual and collective identities in a subject nationality, religion versus secularism, sons against fathers, and above all, peculiarities of a society moving from tradition to modernization.

Heller presents properly organized material, sound judgment and analyses, and uncompromising intellectual honesty.

Knowledge of history, skillful use of archival sources, and familiarity with the literature available are markedly shown. Numerous photographs from the YIVO collection, an elaborate bibliography, and an extensive index complete the usefulness of this book, which I recommend not only to academics but also to the general public, for the author has wisely restricted her use of sociological jargon.

STEPHAN M. HORAK
Eastern Illinois University

N. V. SINITSYNA. *Maksim Grek v Rossii* [Maksim Grek in Russia]. Moscow: Izdatel'stvo "Nauka." 1977. Pp. 331. 1 r. 95 k.

N. V. Sinitsyna has produced the first stage of what one hopes will be a complete, scholarly edition of the works of the Greek scholar and Christian humanist Michael Trivolis, known in Russian history as Maxim the Greek. *Maksim Grek v Rossii* attempts a description and classification of the works of this sixteenth-century writer in the manuscript tradition from the sixteenth through the eighteenth centuries. Sinitsyna emphasizes the relationship the works written by and attributed to Maxim have to the manuscript miscellanies in which they are found.

Maxim the Greek, the most prolific and certainly among the most widely read writers in Russia before Lomonosov, was arrested, tried by ecclesiastical authorities, and sentenced to incarceration in 1525 and again in 1531. Sinitsyna reviews the available evidence concerning

Maxim's downfall and concludes that the "reasons for his conviction were found outside the confines of his original literary works" (p. 145), that is, in his unfortunate involvement in the internal and external affairs of xenophobic Muscovy.

Sinitsyna's work follows in the productive and highly regarded school of textual study and criticism worked out by Soviet literary scholars and historians such as D. S. Likhachev, Ia. S. Lur'e, and A. A. Zimin. She begins with the corpus of Maxim's works including, first of all, the autographs, subjecting them to a thorough analysis with a view to providing a classification, dating, and genre attribution, and to establishing the authorship of a number of works attributed to Maxim in later manuscript tradition. The results of Sinitsyna's analysis, details of which can and will be questioned, show clearly, however, that the questions of classification, dating, and genre are intrinsically linked, and that only a classification based on the manuscript tradition, rather than a thematic arrangement, permits an understanding of the process by which Maxim the Greek developed from a "temporary translator" to a philosopher-teacher to several generations of Muscovites.

Having done the necessary textological spadework to establish a chronology of many of Maxim's most important works, Sinitsyna then logically proceeds to use these data to elucidate Maxim's biography. Her second and third chapters deal with the two periods of Maxim's life in Muscovy (pre- and post-trial) and his literary and related activities during these years. She is thus able to establish probable "provocation" for a number of Maxim's otherwise obscure writings.

Her book concludes with appendixes containing a listing and partial description of the miscellanies of Maxim's works compiled in the sixteenth through eighteenth centuries, a large number of fascinating photographs illustrating this literary heritage, and useful paleographic tables showing the development of Maxim's own hand. Unhappily, but in accordance with standard Soviet practice, the book contains no bibliography. This is certainly an indispensable book for anyone seriously interested in Muscovite literature or cultural history, and it confirms the necessity of a complete command of the relevant archival materials for anyone writing on the subject.

JACK V. HANEY
University of Washington

DEBORAH HARDY. *Petr Tkachev: The Critic as Jacobin*. (Publications on Russia and Eastern Europe of the Institute for Comparative and Foreign Area Studies, number 8.) Seattle: University of Washington Press. 1977. Pp. xiii, 339. \$12.50.

This biography of Petr Nikitich Tkachev (1844–86) is a welcome and valuable contribution to our understanding of the intellectual heritage of the Russian Revolution. Tkachev is the best known of the Russian Jacobins, that is, those who urged the seizure of power by a coup d'état and the employment of that power to transform society. Jacobins were always a minority in the ranks of the revolution, and never more so than during the years 1875–79 when Tkachev was expounding his strategy in the émigré journal, *The Tocsin*. The majority of his contemporaries believed that the direct mobilization of society through agitation or propaganda was the only correct strategy to pursue, and their professions of faith almost always included an explicit rejection of Tkachev for his advocacy of the unworthy and corrupting instrumentalities of political plots and government power.

Deborah Hardy devotes only one chapter to *The Tocsin* and concentrates instead on charting the emergence of Tkachev's views in the legal Russian press. For thirteen years prior to the founding of *The Tocsin*, Tkachev contributed regularly to the leading journals of the day. In the process of evaluating and criticizing Russian and Western writing on a wide range of subjects, he gradually developed his conclusions about the nature of the ideal society and the methods which ought to be employed to attain it. Hardy argues that Tkachev had established the major outlines of his world-view even before his trial and imprisonment in 1869, and that the views he presents in *The Tocsin* are only more explicit restatements of positions he had worked out in his legal articles for *Russian Word*, *Library*, and *The Cause*. The discussion of Tkachev's theory of historical "jumps," which formed the intellectual underpinning of the *Tocsin* program, is especially valuable. With his notion of "jumps" Tkachev sought to square his belief in the basically deterministic development of society with his even more strongly held conviction that individual will and action could be the decisive force in precipitating change. Hardy writes sympathetically of Tkachev's struggles to resolve this irreconcilable dualism, and while she admits that he failed, her treatment invests him with qualities of sincerity and naiveté that none of his contemporaries was able to perceive.

Little is known of the personal life of Tkachev. Even his whereabouts for years at a time have not been established, and the nature of his relationships with family and friends remains very shadowy. Wisely therefore, Hardy has refrained from any psychohistorical speculation and has chosen to base her judgments of the man on his published writings. She generously acknowledges the prior work of Soviet editors and commentators while frequently dissenting from their conclusions. De-

spite her warning in the preface that "educated guesses are prevalent in this manuscript" (p. x), her conclusions throughout appear to be modest, judicious, and amply supported by the sources. The one line of argument that seems strained is her speculation that those elements of the *Tocsin* program that most antagonized its readers—elitism, amorality, and mystification—should not be ascribed to Tkachev but to his sinister collaborator G. M. Turski.

Because of the preoccupation of American scholarship with establishing the relationship between Tkachev's ideas and the tactics employed by Lenin in accomplishing the Bolshevik Revolution, Hardy is more or less bound to discuss this question although she clearly feels it is not the most interesting or productive line of inquiry. Her conclusions are that parallels between Tkachev and Lenin can be drawn but that the evidence will not support the presumption of a causal link.

DONALD SENESE
University of Victoria

WOLFGANG GEIERHOS. *Vera Zasulich und die russische revolutionäre Bewegung*. (Studien zur modernen Geschichte, number 19.) Munich: R. Oldenbourg Verlag. 1977. Pp. 314. DM 78.

In this fine, tautly-reasoned, meticulously-researched study of the Russian revolutionary movement, Wolfgang Geierhos concentrates on the crucial years between the origins of terrorism in Russia (1878) and the beginnings of Russian Marxism in Geneva (1883). His subject is the background, formation, history, and disintegration of the "Black Partition," that often neglected anti-terrorist wing of the revolutionary party which claimed the allegiance of Vera Zasulich, G. V. Plekhanov, Pavel Aksel'rod, and others. We follow Zasulich and friends from her attempted assassination of the governor-general of St. Petersburg through the split over terrorism, the flight abroad, to exile life in Geneva, where they quarreled with terrorists, liberals, nationalists, and each other, only slowly to find their way along the rugged road to Marxism. Geierhos demonstrates that Zasulich was never at heart a terrorist, although her shot at Trepov was the spark that set the movement afire and although she, like many radicals, was caught up in excitement after the tsar's assassination. Still, she continually sought patient and peaceful solutions. When she finally joined the Russian Marxist organization, she was propelled not by the Marx that inspired Plekhanov, but rather by her disillusionment with alternative programs, and by her conviction that Marx himself (as she read his famous letter to her) supported *obshchina* socialism in Russia and believed it could be achieved.

The reader is treated to more than Zasulich herself: to the government's response to her trial and liberation—an excellent study; to tightrope polemics on political and/or social action; to the Marx who could not bear to discourage revolution in any form; to the interminable betrayals, vendettas, and squabbles in the Geneva émigré colony. Indeed, this is not a biography of Zasulich at all. Psychohistorians will look in vain for glimpses into her heart. In many chapters she is scarcely visible. The result is an episodic quality that sometimes leaves one wondering why certain material only obliquely related to Zasulich (the Lev Hartman case, for instance) is included here. Such minor criticism should not detract from Geierhos' overall achievement in this excellent, scholarly study.

The book includes a lengthy bibliography, a checklist of pertinent journals, and a list of nearly a hundred Zasulich writings.

DEBORAH HARDY
University of Wyoming

MARY SCHAEFFER CONROY. *Peter Arkad'evich Stolypin: Practical Politics in Late Tsarist Russia*. Boulder, Colo.: Westview Press, 1977. Pp. iii, 235. \$10.75.

As the last major statesman of Imperial Russia with a definite, clearly articulated political program and the energy and determination to undertake major reforms, P. A. Stolypin has long attracted the attention of historians. Yet Mary Schaeffer Conroy's *Peter Arkad'evich Stolypin: Practical Politics in Late Tsarist Russia* is the first English-language biography of this pivotal political figure who headed the Russian government in the key 1906–11 period and whose enigmatic and untimely assassination may well have ended the last possibility of the peaceful transformation of the country.

As a groundbreaking study of Stolypin, this work makes a number of contributions. Conroy rightfully stresses the importance of Stolypin's provincial experiences—as *uezd* marshal of the nobility and an improving landowner immersed in local affairs—in shaping his subsequent political outlook and his approach to national politics, although this theme could have been developed far more than has been done. Also, while Conroy clearly sympathizes with Stolypin so much that she quite often uncritically accepts as political necessity unsavory aspects of his political career (for example, his sponsorship of the field court-martials in 1906 and his role in promoting Great Russian nationalism after 1909), she nevertheless recognizes that Stolypin achieved few of his political goals; and she maintains that the strength of his political opponents by 1911 was such that it was highly unlikely that Stolypin's reform program

could have been realized in the short run, even had he survived the assassin's bullet.

The discussion of Stolypin's adversaries, however, leaves much to be desired. Readily recognizing the *institutional* obstacles that Stolypin encountered, such as the autocratic powers of the tsar, the legislative powers of the Duma and State Council, and the bureaucracy's role in drafting and implementing legislation, this work fails to cope with the social dynamics of the political process and overlooks the enormous influence of the provincial nobility which dominated the key elective institutions, both national and local, under the Stolypin-created Third of June system. The highly important United Nobility is only mentioned once in passing in small-case letters. This organization, by virtue of its composition, was capable of coordinating the political activities of the provincial nobility, the State Council Right (many of whom were members of the nobles' union), and Stolypin's opponents at Court, in the bureaucracy, and among St. Petersburg society. Without a precise definition of who his enemies were and how they actually operated, Stolypin emerges in these pages as a cardboard Quixote, tilting at shadows, instead of the tragic figure that he really was, defeated politically in large measure by the very social group from which he sprang and with which he continued to identify strongly throughout his career—the noble landowners of the provinces.

This deficiency is compounded by the omission of any detailed discussion of Stolypin's agrarian reforms, by the topical organization of the book which imparts undue consistency to Stolypin's policies, and by inadequate use of available source materials. Relying far too heavily upon memoir literature and the personal impressions of British diplomats, this work consulted the daily press, the single most important source for early twentieth-century Russian political developments, only once and then on a peripheral issue; and no use was made of indispensable secondary sources, such as the excellent biography of Stolypin by the Polish historian Ludwik Bazyłow or V. S. Diakin's 1972 article on Stolypin and the nobility in *Problemy krest'ianskogo zemlevladieniia i vnutrennei politiki Rossii*. As a result of these omissions, Conroy's study, though it is a pioneering work which should be consulted by all those interested in this important statesman, leaves much more to be said of Stolypin.

ROBERTA T. MANNING
Boston College

U. A. SHUSTER. *Peterburgskie rabochie v 1905–1907 gg.* [Petersburg Workers in 1905–07]. Leningrad: Izdatel'stvo "Nauka," Leningradskoe otdelenie, 1976. Pp. 281. 1 r. 46 k.

The history of the working class is a popular subject among Soviet historians, and they are becoming more sophisticated in their treatment of it. This book is a good example of both the virtues and weaknesses of Soviet scholarship in the areas of history and politics.

The first chapter of the book is an admirable summary of information on the background of the working class in St. Petersburg before the 1905 Revolution. It contains a measured and scholarly analysis of the sources for vital statistics, education, wages, and living conditions of workers in the capital. The next chapters discuss the Gaponovite movement, the explosion of unrest resulting from Bloody Sunday, and the tumultuous year of 1905. The book concludes with chapters on 1906 and 1907, and finally a conclusion and historiographic survey. U. A. Shuster organizes a kaleidoscopic treatment of strikes and unrest among the workers around the theme of the leadership of the Bolshevik Party, thereby suggesting a unity of purpose between the two. He does it well and avoids the crude distortions typical of many of his predecessors, but still the work shows the predictable tendentiousness of Soviet historians in dealing with political matters.

Shuster's treatment of the Gaponovite movement is balanced, but biased. He downplays the role of Father Gapon, and exaggerates the roles of both the "opposition" centered around the Karelin (pp. 63-64) and the Social Democrats. He also ascribes too much influence to the Social Democrats in formulating the famous petition of January 9, concluding with a quotation from a letter of a Bolshevik, published by Lenin, to the effect that the petition contained "demands completely corresponding to the minimum program of the RSDRP" (p. 82). The book does not mention that years later the author of that letter, S. I. Gusev, then the secretary of the St. Petersburg Bolshevik Committee, explained that he wrote the now famous line because he had been told that students were able to replace surreptitiously Gapon's petition with one of their own program. This indicates that Bolsheviks in St. Petersburg knew very little of what was happening among the workers during those fateful days. The author concludes that the march to the palace on January 9 was "not a procession to the tsar but a demonstration" (p. 88, italics in the original). This point is difficult to accept.

Similar bias is evident elsewhere. Little space is devoted to the work of the Mensheviks, as for example in the elections to the Shidlovsky Commission, or in the St. Petersburg Soviet where Trotsky played the most prominent role. Although the soviet's influence among the workers is discussed, Trotsky is not mentioned even in passing, while Lenin is constantly held up as an unwavering beacon for the masses.

On the whole, the slant of the book is predictable, but the work is also a good summary of the chronological development of labor unrest and the search for organizational expression among the workers in St. Petersburg during the Revolution of 1905, and for this we can be grateful. It is regrettable that the book does not have an index.

WALTER SABLINSKY
University of Virginia

V. I. KOSTRIKIN. *Zemel'nye komitety v 1917 godu* [Land Committees in 1917]. Moscow: Izdatel'stvo "Nauka." 1975. Pp. 335. 1 r. 20 k.

V. I. Kostrikin's monograph provides the first detailed account of the land committees' role during the agrarian disturbances that shook rural Russia in 1917. Unlike the peasant soviets and ad hoc committees, recently studied by O. N. Moiseeva and G. A. Gerasimenko, these bodies enjoyed legal sanction, but they swiftly came under the control of extremists intent on subverting the feeble Provisional Government and establishing a revolutionary dictatorship. The land committees at the *volost'* (rural district) level were particularly active in harassing, and finally dispossessing, many private farmers—some of them peasants—and redistributing their land or stock on the most egalitarian principles. After the October insurrection the land committees were either suppressed or incorporated into the Bolshevik-controlled rural soviet structure, and the Socialist Revolutionaries (SRs), who had initially wielded the main influence in them, were coerced into acquiescence.

Modern Soviet historians, bound by the canons of "partisanship" (*partiinost'*), can neither acknowledge the SRs' leading role in the countryside during 1917 nor assess their agrarian program fairly. Instead, they present them as "class enemies" and counterrevolutionaries. The line has hardened considerably since the mid-1960s, when at least the Left SRs, who collaborated with the Bolsheviks, received relatively favorable mention. Kostrikin does not appreciate that the orderly land reform advocated by most SRs was a viable, indeed preferable, alternative to the anarchic free-for-all that eventually took place with the extremist parties' sanction. Less excusable is his failure to admit that the Bolsheviks themselves were ambivalent about dividing up the advanced estates, which was bound to reduce agricultural output and foster proprietorial attitudes. Lenin's endorsement of such action was a tactical maneuver designed to win immediate support from the new small-holders, who were then to be dispossessed in turn by their fellow villagers in a new round of "class struggle." Ironically, Lenin's Machiavellian master stroke cannot be discussed frankly by his

latter-day disciples, who are obliged to present the Bolsheviks as absolutely consistent friends of all "toiling" peasants.

Kostrikin's ideological stance deprives his thesis of any claim to be considered seriously. His work's significance lies in the factual data it contains on the land committees' organization and activities. We are given a statistical analysis of the measures taken by 1,088 of them (only one-fifth of the total, for reasons not satisfactorily explained), but unfortunately the loose categories employed detract from the value of this table. More serious is the failure to relate these measures (e.g. rent control, land seizures) to the local socioeconomic context, and so to clarify their actual cause and effect. This task could have been attempted on the basis of the archival material consulted here, but alas, party dogma rules that class affiliation alone is of consequence. Instead, Kostrikin is content to amass evidence indiscriminately from various regions in order to illustrate the peasants' militancy, and then to hail this as proof of popular support for Bolshevism. This study, a characteristic product of present-day Soviet historical scholarship on the Russian Revolution, is much inferior in quality to works published in the 1920s.

JOHN KEEP
University of Toronto

V. P. VERKHOS'. *Krasnaia gvardiia v Oktiabr'skoi revoliutsii* [The Red Guard in the October Revolution]. Moscow: Izdatel'stvo "Mysl'." 1976. Pp. 262. 1 r. 11 k.

One trend in the writing of Soviet history of the post-Stalin era has been to focus more attention on the role of lesser figures and on a greater variety of social groupings and organizations. The result has enriched Soviet histories and made available source materials which allow Western writers also to get away from focusing overmuch on the most prominent figures. This has been especially true in the study of the Russian Revolution of 1917, where the past decade and a half has seen an increase in books devoted to the study of the workers, various lower-level organizations, and the Revolution in the provinces. The Red Guard has been the subject of several specialized works: V. I. Startsev's study of the Petrograd workers' militia and Red Guard is one of the best Soviet studies on the Revolution. Now V. P. Verkhos' has undertaken to provide a summary history of the Red Guard across all of Russia.

Verkhos' work is rather more tendentious than the best recent Soviet historical writing. He apparently belongs to that school of Soviet historians who feel the need constantly to assert Bolshevik leadership and influence, even more than required

by Soviet rules of historical writing. That, however, is common enough to be readily coped with. More important is how Verkhos' tries to deal with the problem of writing the history of one group or phenomenon across the diverse face of Russia. He opts for an eclectic approach which draws a bit of information here, a bit somewhere else, yet another piece elsewhere, in order to patch together a collage portrait. It does not work well. The result is that the Red Guard fails to come alive, the sense of development and growth is lost, local variations are smoothed over, and one does not get a picture of the real, actual Red Guard. The eclectic approach is especially difficult when dealing with a phenomenon which was local in origins and lacked any centralized structure or leadership, and Verkhos' fails to pull it together adequately.

The book does, however, provide some useful data on the Red Guard and provides some sense of the breadth of the movement. The factual data are generally reliable, if incomplete, but one must be cautious in using the conclusions Verkhos' draws. The book contains the usual denigration and distortion of the role of other political, especially socialist, parties. One special warning: the charts at the end seriously undercalculate the total size of the Red Guard. His figures are too low for some cities (Saratov for instance), and there are none given for October for some provinces with major cities that certainly had Red Guard detachments. Nor is allowance made for Red Guard units that existed but about which no records survive. As this is the most impressive effort to bring together in one place the data on size, even if based on an assortment of inadequate sources, it will be tempting for historians to use his totals uncritically. One hopes that will be avoided.

REX WADE
University of Hawaii

MICHAEL PALIJ. *The Anarchism of Nestor Makhno, 1918-1921: An Aspect of the Ukrainian Revolution*. (Publications on Russia and Eastern Europe of the Institute for Comparative and Foreign Area Studies, number 7.) Seattle: University of Washington Press. 1977. Pp. xii, 428. \$14.50.

The Ukrainian Revolution of 1917-21 was a complex, confusing, and ultimately tragic sequence of events that usually defies rational and systematic exposition by the historian. Following the collapse of tsarist authority, the left-of-center, reform-minded Central Rada sought to establish Ukrainian independence only to fall victim to Soviet and then German intervention in April 1918. The more conservative Hetmanate régime of General Skoropads'kyi lasted only as long as its German protectors were involved in the greater events of the

World War. In December 1918 it too was overthrown by a coalition Directory which sought to pursue ill-defined nationalist and socialist objectives in face of renewed Bolshevik, White Russian, French, and Polish intervention. It is not surprising that this lack of firm central authority and the obvious need for protection against outside invaders gave rise to numerous armed partisan bands whose allegiance shifted with military fortunes and their own changing self-interests. The most important of these partisan movements was led by Nestor Makhno, who at one time controlled an area in southeast Ukraine the size of the Netherlands and, with an army of twenty-five thousand men, played a decisive role in the defeat of the White Generals Denikin and Wrangel.

Michael Palij's book is the first major Western study of the Makhno movement and is based on a thorough reading of Soviet, émigré, and Western literature. His one hundred fifty pages of notes and annotated bibliography, which represent the most valuable part of the book, will be of very considerable use to future scholars of the Ukrainian Revolution. As the title of his book implies, Palij seeks to examine three facets of the movement. First, he is interested in Makhno's "anarchism," but soon comes to the conclusion that "he was not of much account as an anarchist theorist" (p. 61). Indeed, he "had no positive goal" and never "worked out a plan for . . . a stateless society" (p. 252). Thus his "anarchism," unless it is defined solely as his activity as a bandit leader and sometime assassin, becomes something of a nonsubject. On a second level, this is a biography of Makhno himself, concentrating on his military exploits during the period from his return to the Ukraine after the February Revolution to his defeat by the Red Army in 1921. Here, too, the subject undermines the author. The lack of material on Makhno's childhood in the Ukraine and his early manhood in a Moscow prison limits his pre-1918 biography to a scant three pages. A similar (and insufficient) number of pages is devoted to Makhno's unhappy life after 1921 when he wandered from Romanian interment to Polish imprisonment to Paris emigration and death in 1935. On a third level, Palij tells in more detail than is necessary the familiar if confusing story of the birth and death of the Ukrainian state. In this context, he faults Makhno for his lack of national consciousness, his failure to understand the aspirations of the republic, and his indirect contribution "to the triumph of bolshevism" (p. 252).

The tripartite nature of the author's undertaking leads at times to further confusion and to repetition. His prose does not always clarify matters (e.g. "Although in the early days of the coup the hetman 'had a clear intention to give the government a national Ukrainian character,' by inviting

the leaders of parties to enter the government and administration, they chose to form an opposition rather than to accept the hetman's invitation" [p. 117]). In sum, this is a conscientious doctoral dissertation whose very subject probably precluded its being turned into a truly successful book.

R. C. ELWOOD
Carleton University

Z. V. STEPANOV. *Kul'turnaia zhizn' Leningrada 20-kh—nachala 30-kh godov* [The Cultural Life of Leningrad in the Twenties and Early Thirties]. Leningrad: Izdatel'stvo "Nauka," Leningradskoe otdelenie. 1976. Pp. 286. 1 r. 50 k.

"Art to the masses" has been a recurring motif in Soviet cultural policy over the past sixty years. But, while mass accessibility has always been a criterion of artistic success, the judgment on accessibility has usually been intuitive; and it has been related to a value judgment on the *ideinost'* (literally, "idea-content") of the given work of art.

Western scholars have tended, rather misleadingly, to equate the Soviet *ideinost'* requirement with an ideological imperative. In fact, it often has little to do with ideology, and a great deal to do with traditional Russian assumptions about culture and *kul'turnost'*, or cultural respectability. A Tchaikovsky symphony scores high on *ideinost'*, while a James Bond movie scores very low. The Soviet public has never been exposed to such basic ingredients of Western popular culture as pornography, violence, or even True Romance (this is one of the major, though least remarked, achievements of Soviet censorship). But, in the hypothetical situation in which Soviet censors had to distinguish between acceptable and unacceptable forms of pornographic literature, they would surely use the criterion of *ideinost'* as an almost exact equivalent of our "socially-redeeming value."

In Soviet terms, Z. V. Stepanov's book is unusual because it focuses not only on the makers of cultural products but also on the consumers. He seeks to describe and, where possible, measure audience response to the theater, music, art exhibits, cinema, and radio programs available in Leningrad in the 1920s and early 1930s. He tries to establish the social composition of the audience and, in particular, the degree of working-class interest in different forms of art.

Stepanov is not so much writing a history of popular culture in the Western sense as a history of the popular response to high culture. Like all Soviet writers, he distinguishes between "culture," which is good and at least ought to be popular, and "*khaltura*" (cheap or low-brow culture), which is popular but bad. *Khaltura* lacks the *ideinost'* of

true culture. To quote Stepanov on some of the theater repertoire of the 1920s, it has "a purely entertainment character." Following the conventional Soviet treatment of the theme, Stepanov suggests that the working-class public of the 1920s appreciated true culture, while the non-proletarian "petty-bourgeois" public provided the audience for *khaltura*.

A Western sociologist would find this hypothesis unlikely, but there is always the possibility that it might be true, or at least true of the elite of upwardly aspiring industrial workers who were drawn toward the Soviet Communist Party in the 1920s. Stepanov, however, has not made a very convincing case. His statistical data on theater attendance are difficult to interpret, since the "culture" theaters distributed large numbers of reduced-price tickets to trade unions and other organizations, while the "*khaltura*" theaters depended solely on box-office sales. The social breakdowns of audience which he quotes from small-scale surveys of the 1920s cannot be taken at face value. RAPP, the Russian Association of Proletarian Writers, which inspired most of the survey-taking in this field, was interested in demonstrating working-class support for the artistic enterprises which it happened to approve; and the RAPP ideology excluded the possibility that the working class might choose *khaltura* over culture.

Despite its deficiencies, Stepanov's book is a valuable attempt to extend the traditional boundaries of Soviet cultural history. Given the data problems of the 1920s, it might have been more profitable to ask the same questions about audience response in a contemporary context. But there are researchable topics on the 1920s which relate to Stepanov's interests: one obvious example is the effect of Soviet import policy during NEP and the First Five-Year Plan both on the development of the Soviet film industry and the taste of the mass audience which, as Stepanov indicates, preferred cinema to all other arts. Cultural sociology is a new field for Soviet scholars (and for Western scholars of the Soviet Union), and we may hope for some interesting developments in the coming years.

SHEILA FITZPATRICK
Columbia University

NEAR EAST

WILLIAM W. HADDAD and WILLIAM OCHSENWALD, editors. *Nationalism in a Non-National State: The Dissolution of the Ottoman Empire*. Columbus: Ohio State University Press. 1977. Pp. x, 297. \$15.00.

The Ottoman Empire is yet to find its Oscar Jaszi, but the idea behind this book, which is a festschrift

for Sidney Nettleton Fisher, has at least a resemblance to the idea behind Jaszi's classic about the Habsburg Empire. Its purpose is to evaluate the sources of strength of the national movements in the Ottoman Empire and to assess their contribution to its demise. The conclusion, stated by William W. Haddad in the introduction, is that the European provinces first discovered nationalism and this resulted in their separation from Istanbul. The Arab revolts of the early twentieth century were not, however, primarily nationalist movements, and North African nationalism did not play a role in the dissolution of the empire.

Unfortunately, since the book is not a study, but rather a collection of brief articles on topics of special interest, it falls short of providing satisfying answers to the questions it poses. An exception is the article by Roderic Davison, in which he discusses the reactions of the Ottoman Empire to the dangers it faced in the nineteenth century. The Ottomans tried consciously both to accommodate and to resist the threat of nationalism, perhaps even more consciously than did the Habsburgs, even though the Habsburgs understood the nature of the threat more clearly than did the Turks. Davison discusses seven devices the Ottomans used, and points out that an eighth, federalism, was not tried because the millet system, appropriate for a mosaic state, could not be translated into the sort of territorial organization demanded by national ideology.

Because so much effort has previously been concentrated on the European provinces, only one article in this collection concerns that area—Carole Rogel's only partially successful effort to show that what she calls the Wandering Monk created a more worldly intellectual climate in Serbian lands in the eighteenth century. In another paper about the eighteenth century, Alan Fischer shows that the Crimean Khans were autonomous and even sovereign from the sixteenth through the eighteenth centuries. By promising to increase this special status to complete independence in the late eighteenth century, the Russians were able to separate the Khans from the empire and absorb the Crimea.

Most of the rest of the articles concern the twentieth century. William Spencer succeeds in showing how the varying circumstances by which the Ottoman Empire acquired Algiers, Tunis, and Tripoli were preserved by Ottoman rule and thereby became the basis for three separate modern national movements, but only after the end of the empire. Russell Yates Smith describes in what way the career of Sa'd Zaghlul in Egypt before World War I was a prelude to his nationalist period.

In their discussions of Arab and Palestinian national movements, Rashid Khalide and Ann Lesch

both stress the lack of any organized Arab movement until the Young Turk revolt in 1908. By establishing an assembly, the Committee of Union and Progress provided an opportunity for Arab political opposition to form. But that opposition did not have a national basis. Emotions may have been aroused when the CUP changed the official language from Arabic to Turkish, but the real basis of early Arab political consciousness was anti-Zionism. In the first decade of the twentieth century Jews from Europe were buying land in geographic Syria, often in large tracts from landlords who had acquired it from impoverished Arabs late in the nineteenth century. The possibility of wholesale alienation of Arab lands to the Jews is what inspired political agitation among some of the formerly inert Arab leaders as they realized that the Moslem Millet was rapidly becoming a Turkish Millet, unresponsive to Arab problems.

Despite the interest of some of the contributions, in the end this book is not very satisfying. The introduction is not as penetrating or as theoretically sophisticated as one would like, some of the articles are quite narrow, and the papers seem to be written with varying purposes in mind. These problems aside, however, the book demonstrates how difficult it would be to write a comprehensive study of the dissolution of the Ottoman Empire; it indicates that a simple transferral of European categories will not work, and suggests that a major collaborative effort to write an integrated study may be called for.

GALE STOKES
Rice University

A. YODFAT and M. ABIR. *In the Direction of the Persian Gulf: The Soviet Union and the Persian Gulf*. Totowa, N.J.: Frank Cass. 1977. Pp. xii, 167. \$19.50.

This slim yet sometimes masterful volume represents the combined efforts of two Israeli specialists: Mordechai Abir (author of *Oil, Power and Politics* [1974]) and Aryeh Yodfat (*Arab Politics in the Soviet Mirror* [1973]). The product of their expertise, however, is not quite as clear as is the route taken by the great power under examination.

This chronological investigation of Russian interest in the Persian Gulf—from Muscovy to MIRV missiles—is developed through a number of brilliant analyses, which are often lost among descriptions of conflicting nationalist movements, army coups, political assassinations, great power pressures, and the heady influence of petrodollars. One dreads to think that the region's conflicts are as complicated as the narrative. Few paragraphs, for instance, touch upon less than five intertwined

events or countries; and the book, despite its modest length, contains enough abbreviations of political parties, splinter groups, and popular liberation organizations to justify the supplementary page of definitions. Moreover, only three unexplained and sketchy maps are provided to help the reader through the maze of events; and, while the authors have made admirable use of Soviet, Arab, and Western broadcasts and press reports, no reference is made to a number of important studies, several indeed from Tel Aviv University's prestigious Shiloah Institute.

Despite these problems, the book expertly describes the Soviet Union's dogged expansion down the Persian Gulf, past Saudi Arabia, Yemen, Aden, Somaliland, and out into the Indian Ocean. The final two chapters summarize Russia's achievements and incisively catalogue her long-term, intermediate, and short-term aims in the region. America's alternatives are also analyzed, and, in one of the first such applications to the Middle East, the erosion of American credibility as a result of the Vietnam War and Angola is carefully considered. Equally intriguing is the conclusion that Kissinger's policies in the area "may be seen as a continuation of the policy of the late . . . John Foster Dulles" (p. 131). The book closes, as it opened, on the subject of oil as a major factor in Russia's southward expansion, thereby completing a complex but nonetheless significant initial investigation into the Soviet Union's new playground, the Persian Gulf.

ARNOLD KRAMMER
Texas A & M University

GARY S. SCHIFF. *Tradition and Politics: The Religious Parties of Israel*. (The Modern Middle East Series, number 9.) Detroit: Wayne State University Press. 1977. Pp. 267. \$14.95.

One of the most vexing problems in the internal politics of Israel is represented by the existence of religious parties whose loyalties belong primarily to the *Halakha*, traditional Jewish religious law, and only secondarily to the state of Israel. When, in the view of the leaders of the religious parties, a law passed by the Knesset (the parliament of Israel) conflicts with rulings of the *Halakha*, they feel that it is their religious duty to disobey or disregard it. On the other hand, they are also convinced that it is their God-given duty to coerce the nonreligious majority of the Jewish population to observe the *Halakha*, especially in public life.

All the religious parties together—there are several, differing from one another in the degree of their observance—have obtained only twelve to fifteen percent of the total votes cast in any of the

national elections during the thirty years of Israel's existence. Yet the political configuration is such that the party which obtained the plurality of votes (Labor until 1977, and most recently the right-wing Likud Party) had to form coalitions with the major religious parties in order to secure a working majority in the Knesset. This, in turn, gave the religious parties a strong bargaining position in the government and enabled them to have the Knesset pass religiously motivated laws which went counter to the nonreligious majority in both the Knesset and the population at large.

This, in a nutshell, is the background against which must be viewed the development, structure, and functioning of the religious parties in Israel. Gary S. Schiff, in his *Tradition and Politics*, presents a brief history of the religious parties from the earliest Zionist congresses in the late nineteenth century, followed by a more detailed analysis of their organization, membership, leadership, and share in the governmental system of Israel. He devotes much attention to what he terms the "nationalization of religion," i.e., the absorption into Israel's institutional and fiscal framework of the religious structures and procedures which were developed by the Jewish community in Mandatory Palestine up to 1948. As the author puts it, "Mizrahi and ha-Po'el ha-Mizrahi (later Mafdal, the National Religious Party), true to their traditional pattern of behavior, accomplished [these] goals incrementally, deftly exploiting their pivotal coalition position as the most convenient partner for Mapai-Labor" (p. 151). As early as 1953, the religious parties succeeded in having the Knesset pass the "Rabbinical Courts Law (Marriage and Divorce)" which, among other things, has made it impossible for an Israeli to marry in Israel a member of a different religion. This was followed by the "Dayanim Law" of 1955 which enabled the Dayanim (religious judges) to adjudicate cases on the basis of the *Halakha* and not on that of the law of the state in general (p. 160).

A striking example of unsuccessful efforts by the religious parties to bend the Knesset to their will is the Sabbath rest law. In the 1950s and 1960s, the Mafdal made repeated attempts to have a law passed which would make it a violation to operate businesses, including gasoline stations, restaurants, places of entertainment, and public transportation on Sabbaths and holidays. No such law has been passed, and the regulation of Sabbath rest remained within the jurisdiction of local municipalities and councils (pp. 165 ff.).

Education is a field of governmental activity with which the Mafdal has been most keenly concerned. "The contest for the immigrants," as Schiff calls it (p. 174), has been going on ever since 1948, with religious education as its focus. To

judge from the 1953-75 statistics, religious state education has lost some ground relative to general state education: in those twenty-two years the number of pupils in religious primary state schools dropped from 24.5 percent of the total to 23.3 percent, while that in the state's general primary schools rose from 68.5 to 70.7 percent (p. 180).

A crucial issue for the religious parties in Israel is whether, according to the *Halakha*, religious Jews are or are not allowed to cooperate with a nonreligious government. Mafdal has answered this question in the affirmative, and consequently, was able to use its position as a coalition partner to press, often successfully, for religious legislation. Other, smaller religious parties, such as the Agudat Israel, participated only after considerable hesitation, and one splinter group, the Neturei Karta, still regards the very existence of Israel as a sacrilege.

These issues, and many more (such as the question of who is a Jew) are dealt with by Schiff, himself an observant Jew, in a balanced, objective manner, which makes his book a valuable contribution to the problem of state and religion in Israel.

RAPHAEL PATAI
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AFRICA

ROBERT S. SMITH. *Warfare and Diplomacy in Pre-Colonial West Africa*. (Studies in African History, number 15.) London: Methuen; distributed by Harper and Row, New York. 1976. Pp. 240. £5.50.

Robert S. Smith has set himself the task not so much of presenting new material as of consolidating most of the research that has already been accomplished in relation to his topic. He has succeeded well, and in doing so, he has performed a signal service.

Yet, I must confess that I approached this book with a sense of dismay which never entirely left me! Two-hundred pages of closely packed small print in a volume only $4\frac{1}{2} \times 7\frac{1}{4}$ inches in size cry out for some relief, and this could so easily have been provided, if not by the occasional photograph or other illustration, then by very necessary maps, the total absence of which is utterly regrettable. Difficulty of reading the type and the fact that the notes are all at the end of the book are deficiencies one would hope to see avoided but which present-day costs make increasingly likely to be encountered.

But the work itself is meritorious. Its bibliography is excellent, its scholarship erudite, and its

material fascinating—though for the reasons given it does not “grip” the reader to the extent that one could wish. And to me, perhaps too much of the purist, there are other minor irritations. Why be unnecessarily prolix? The Victorian “diplomats” for “diplomats,” and, horror of horrors, “periodizations”!

To address the material more critically: is Smith correct that the institution of “credit” and “accountancy” in West Africa is *really* to be attributed to European influence? The Arab culture raised both to the status of an art, and Arabs in trade go back well over a thousand years in West Africa. His failure to provide us (even in the notes) with any translation of his Latin legal maxims (p. 31) or even of the Italian of the unnamed Renaissance Ambassador (p. 22) smacks of intellectual snobbery. On the subject of ambassadors, it is also surprising that in the quite lengthy passage devoted to their exchange, there is no mention of Bornu’s resident envoy in Kano, though his house, the *Gidan Shettima* still stands, across from the Mosque, as the oldest building in that ancient city.

Some points which Smith does raise also appear to be rather inadequately handled. He is clearly not at ease with the Hausa concepts *shigege* and *kad’o* (p. 39). What was it that accounted for the inexorable march *eastward* of the centers of the successive empires of the savannah from Kumbi to Gao, and later to Sokoto (p. 44)? Why was the role of the Armah (Rumah) less than adequately discussed (p. 46)? What of the true role of the cavalry in the army of the Jihad (p. 64)? Was a chief’s storage of poisoned arrows really based on economics (as Smith suggests), or was there not a much more potent reason, namely, that *dafi* (arrow poison) could only be made during the wet season, when the botanical ingredients were “ripe,” and that it therefore had to be set aside under suitably controlled arrangements if it was going to be used at all (p. 92)? Moreover, the expenditure of arrows, during a siege for example, was monumental and an adequate strategic reserve was imperative.

None of these strictures, however, should detract from what Smith has accomplished. He has produced a solid, authoritative, and genuinely comprehensive work which ought to find a ready acceptance among scholars and should do much to advance our knowledge of his field. Fascinatingly, also, he has drawn attention to yet another echo of the presence of “Americans” in the Niger basin, this time ca. 1851, an occurrence I have also noted, but some thirty years earlier (Johnston and Muffett, *Denham in Bornu* [1973], 247–50). A dissertation topic stemming from these references would bring on the need for a total rethinking of the whole chronology of “Western” intrusion in the Niger Valley. Is there no one to attempt it?

Not to carp, but to regret, the Hausa for the “time for sharpening knives” (p. 53) is more correctly *washin wuk’a not wasan wuka*. The *malafa* (straw hat) was worn *over* the turban, not under it (p. 103). Why was the existence of the “terraced fortress” (so signally exemplified—and projected into the twentieth century—by the Gwoza Hill) ignored in an otherwise well-organized consideration of static warfare and fortifications?

All of these, however, constitute reservations (nothing more) on the part of an interested reader. Who of us, if any, is innocent of similar failings in our own endeavors?

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KENNETH C. WYLIE. *The Political Kingdoms of the Temne: Temne Government in Sierra Leone, 1825–1910*. New York: Africana Publishing Company/Holmes and Meier. 1977. Pp. xviii, 251. \$27.50.

In his novel *Things Fall Apart*, the Nigerian author Chinua Achebe described the devastating impact of Britain’s presence on traditional life among the Ibo. Ancient customs and prerogatives, religious practices and sanctions, land usage and economic systems, all were undermined by the interference of imperialism and, ultimately, were replaced by alien institutions.

It is Kenneth Wylie’s thesis that things did not “fall apart” for the Temne of Sierra Leone. The Temne had a long history of external pressure prior to the treaty of 1825 which began the process of turning their chiefs into agents for the British. Beginning in the sixteenth century they were invaded, subordinated, and occasionally ruled, by Mande-speaking peoples on their northern frontier. Islamic influences penetrated from the north at the same time as European and Christian influences were coming from the Atlantic coast. Located astride caravan routes connecting the upper Niger to the coast, the Temne were in constant contact with different political systems and were frequently brought into commercial relationships established by outsiders. The result, according to Wylie, was an internal dynamic in Temne institutions providing for adaptation and accommodation to external interferences. The gradual and increasing British influence from 1825 to 1896, when the Protectorate was declared, was thus no shock to the Temne. Their chiefs, like the traditional officials of Iboland, did indeed find themselves without their rights to collect tribute, to maintain justice, to make war and to distribute wealth, yet the chiefly position was retained as an influential institution and as a traditional focus

capable of preserving Temne dignity during the period of colonial control. The Temne, both chiefs and people, were used to institutional change. Imperial encroachment could be regarded as a stage in a continuum, albeit a dramatic one, rather than as a revolution.

The contribution of Wylie's study is to that growing school of historiography which refuses to regard colonialized peoples exclusively as political subjects and economic objects of the Europeans. In a densely argued and detailed description which, while concentrating on domestic political changes, ranges over Temne involvement in the slave trade, "legitimate commerce," regional warfare, diplomacy, and crop production, Wylie demonstrates that the Temne were neither passive pawns nor simple victims. Although they were caught in a situation that was extremely limiting, the Temne continued to respond and to take initiatives according to a system which had by then become indigenous. Wylie reminds his readers that African history was not static before the arrival of the Europeans, and that the advance of colonialism involved a relationship between peoples and their institutions as well as a structural imposition.

The absence of a narrative line limits this book's usefulness to those who are already familiar with the main outlines of Sierra Leone history, though a set of historical maps does provide assistance in following the details of the text. Temne opposition to the Sierra Leone Colony between 1787 and 1807 is unfortunately overlooked, and the Hut Tax War of 1898 is treated summarily. Still Wylie's conclusion that accommodation could be as heroic and dignified as armed resistance is a serious interpretation attracting wider application in the study of the impact of empire. Perhaps Achebe's hero, Okonkwo, sacrificed his life in vain.

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JOHN A. WORKS, JR. *Pilgrims in a Strange Land: Hausa Communities in Chad*. New York: Columbia University Press. 1976. Pp. xiv, 280. \$15.00.

The basic concern of the book is with the twentieth-century formation of Hausa communities in Chad, and in particular, though by no means exclusively, in Fort Lamy and Abéché. The Hausa in Chad are part of the wider Hausa diaspora, found in many parts of Africa outside the Hausa heartland; but, whereas the mainspring for such migration was usually trade, in the Chad area it was the pilgrimage. "It is the administration, economy, and pursuance of the hajj which has become the organizing principle for the Chadian Hausa, tran-

sients and settlers alike" (p. xi). The first chapter discusses Hausa migration and pilgrimage in general; then come specific studies of the Hausa in Abéché and Fort Lamy—very detailed urban analysis; a chapter on Chadian Islam and another on the economic expansion of the Hausa pilgrim communities complete the body of the book. Although Works concentrates on the present century, there is a good deal of evidence also from the nineteenth. The material is attractively, sometimes even vividly, presented. The pilgrimage has long been recognized as of vital significance for the development of Islam in black Africa: almost for the first time, this book explores the remarkable ramifications of at least one pilgrimage route. Works contributes substantially to our understanding of Islam south of the Sahara.

Quite apart from the pilgrimage in particular, the real importance of the transcontinental route south of the Sahara remains a fascinating problem in black African history. Works found it difficult to gather much information about this route in the precolonial period; and I found it difficult to check the references he does give, for he sometimes uses less accessible editions (Burckhardt's *Nubia* in the first edition rather than the second, Denham and Clapperton in their Boston 1826 garb, rather than the recent scholarly Hakluyt version). One or two details might be queried: a camel, for example, was not essential for the pilgrim intending to cross the Sahara (p. 9): many travelers—Nachtigal, and Vischer, and others—traversed the desert in company with pilgrims on foot. Nevertheless, although the early period, even the early nineteenth century, remains obscure, it is clear that later travel on this highway was lively enough.

One very interesting theme, present but only partly explicit, in the book is the practical value of these very slowly moving pilgrimage populations. From the point of view of the sending government, the pilgrimage might seem a brain-and-brawn drain, as skilled personnel, taxpayers, food-producers, potential recruits, and the like, departed. Hence there was sometimes an understandable, albeit theologically embarrassing, reluctance to let pilgrims, or at least large numbers of them, set out. Works develops one case, that of the Dassanawa, of pilgrims departing against the wishes of their rulers, and my hunch is that a lot more similar evidence might be unearthed. The other side of the coin is the welcome which such immigrants received, in the countries which they visited; this is very well brought out here.

On the strictly religious side, our ostensibly rational age consistently underrates the importance of the Muslim cleric as thaumaturge, as the practitioner of religious, perhaps semi-magical, arts. Works is no exception. He mentions some of the

main headings of such activity—dream interpretation, horoscopes, charms, etc.—but without development. Elsewhere the cleric's services for his patron are summed up as "teaching and spiritual counsel" (p. 105), or the sultan is said to have "appreciated the prestige of the cosmopolitan 'ulama'" who frequented his court (p. 133). This relative neglect of Islamic clerical activity means that an important element is lacking in the section about "noncommercial economic activity" (p. 206 ff.), where payment for clerical services rendered surely deserves a place. More important, this relative neglect partially obscures the role of the Hausa in religious change, a role for which Works indicates they seem "particularly suited" (p. 161).

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ANDREW ROBERTS. *A History of Zambia*. New York: Africana Publishing Company/Holmes and Meier. 1976. Pp. xv, 288. \$17.00.

Andrew Roberts has written a well-balanced, incisive, comprehensive and authoritative general history of Zambia which is likely to be long unsurpassed both for Zambia and as a model for treating other African countries. The only major weakness, which Roberts justifies, is one of balance, with a more detailed treatment of the precolonial eras and a selective handling of the colonial and independence periods. When measured against the work's strengths of accuracy, a wide view of what history is, a clear handling of the frequently complex interaction of factors of change and an awareness of the continuity of themes now of current importance, the faults of emphasis or detailed omission are very minor.

In such a general work one's desire for more frequent asides as to the inadequacy and bias of sources and the need for more research and discussion of conflicting interpretations cannot be met. Likewise, one's hopes for a comparative approach are usually doomed to frustration. In both respects, however, Roberts does just enough to inspire confidence. In three detailed chapters on prehistory Roberts provides a long-needed updated synthesis. His emphasis on continuing patterns of importance makes a remote period more relevant.

Roberts is best in dealing with the precolonial period in the four chapters on Zambian peoples to 1700, the growth of chieftainship from 1700 to 1840, the expansion of trade to 1840, and African intruders from 1840 to 1890. Here he wisely chooses a more analytical and topical approach. Especially important is the section on trade to 1840, both for its detail and interpretations of reasons for changes

and the influence on politics. This period before about 1840, which still saw much local initiative, was a transition to the next half-century of military and economic aggression from all directions which devastated many areas, benefited a few areas and left no area unchanged. Although there is superb analysis of factors of change, more attention to mechanisms which previously had assured stability or slow change and why some institutions adapted and survived might have helped.

The author's discussion of the British takeover is a fine example of able condensing of the process, while the following chapters on the colonial era are highly selective and general. Any of the important themes of labor migration, economic change, urban growth, European settlement, and the development of African expression, eventually to nationalism, could have been much fleshed out. It was annoying to the reviewer to think on one page that an important point had been missed, only to find it made a few pages later, and often with more telling effect than when originally expected. Although Roberts' handling of the main points is adequate it is skeletal, and many minor points were scanted. But Roberts did not intend otherwise, and within his set goals he cannot be faulted.

The ten years since independence are well covered in a critical yet sympathetic assessment. Although Roberts ably summarizes a number of important themes, including ethnic rivalry, national integration, economic development (yet economic dependence), problems of relations with southern Africa and internal politics, it might have helped if differences in interpretation were more openly identified.

The physical presentation of the book is fairly good. Although few, the illustrations are well done, especially those by the author. The maps are clear, well related to the text and with only negligible mistakes or omissions. The bibliography is extremely useful, although Roberts might be slighting the *International Journal of African Historical Studies* by omitting it from a list of journals with occasional articles on Zambia. The only serious flaw—no doubt the publisher's fault—is the microscopic print in the nine-page index.

With few exceptions, and none of those very important, Roberts has done a fine job of presenting Zambia's past to any audience, whether the scholar, the student, or the general reader.

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OBARO IKIME. *The Fall of Nigeria: The British Conquest*. New York: Africana Publishing Company/Holmes and Meier Publishers. 1977. Pp. xi, 232. \$12.00.

Obaro Ikime is well known for his work on the Niger delta, on leadership in Africa, on West African chiefs, and on resistance. Perhaps less well known are his many contributions to the *Journal of the Historical Society of Nigeria* and other Nigerian publications. His accomplishments during the past decade have in fact been prodigious, and they have been of the high standard associated with the "Ibadan school." He has also gone beyond the production of scholarly studies to a concern for the general reading public, an area—alas—in which few professional historians venture.

Ikime's most recent volume falls under this category, for in content and style it is aimed at a wide audience and represents a synthesis of contemporary scholarship on the British conquest of Nigeria, a topic that continues to receive attention. The work is divided into two parts: the first describes the changing nature of British-Nigerian relations in the nineteenth century and the second contains brief essays entitled "The Fall of Lagos," "The Fall of Oyo," "The Fall of Brass," and so on, an essay for each of the twelve states that constituted Nigeria when the book was being written. Southern Nigeria receives the greater emphasis, as may be expected, and footnotes are dispensed with in favor of bibliographic notes at the end of each chapter. There is a sparse "select bibliography" and the book contains a number of rare early-twentieth-century photographs associated with the British conquest.

It is inevitable in a work such as this that there would be interpretations and generalizations open to question. References are made to Christian missionaries being welcomed in Abeokuta in the 1840s, for example, when in fact the political jockeying in that Egba citadel between civil and military factions after the death of Sodeke resulted in the missionaries cooling their heels at Badagri. Their ultimate reception at Abeokuta, by the *ologorun* especially, was something less than a welcome. Another example is the statement that the bombardment of Lagos in 1851 was the result of Britain's desire for a share of the trade from which it had been excluded. What trade? Legitimate commerce along that stretch of the coast was notoriously poor. The volume of palm oil was but a trickle, amounting to less than 4,000 tons even in 1856 and it remained depressingly low for decades. The evidence instead suggests that the Lagos bombardment was inspired by the Church Missionary Society (C.M.S.) as part of its anti-slave-trade and pro-Egba policies.

A few errors have slipped through. The C.M.S. is called the Christian Missionary Society (pp. 8, 10); Owu, Akure, and Ekiti are mentioned as belonging to Ibadan's *western* Yorubaland empire (p. 149), and there is the puzzling reference to the

openness of Badagri harbor (p. 95). But these are minor points that will not negate the book's usefulness to the non-specialist reader.

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BALA PILLAY. *British Indians in the Transvaal: Trade, Politics and Imperial Relations, 1885-1906*. New York: Longman. 1976. Pp. xvii, 259. \$10.00.

The study of Indians overseas has for some time been an academic niche. While anthropologists tend to look at changes in caste, historians are most frequently concerned with the political relationship between Great Britain, the Government of India, and the host country or colony. Overseas Indians were unique in that they were non-white British subjects who claimed the rights of British citizens.

Bala Pillay's *British Indians in the Transvaal* is firmly in the historical tradition. It is a straightforward, unimaginative but competent account of one aspect of the complex relationship between three parts of the British Empire during the years 1885 to 1906, from the passing of the first major discriminatory legislation against Indians to the achievement of responsible government.

While most studies of Indians in South Africa have concentrated on Natal and the Cape, the areas of greatest Indian population in South Africa, a study of the Boer republic of the Transvaal can provide an interesting counterpoint. Although Great Britain clearly had imperial interests, the South African Republic was an independent state until 1902, often hostile to the British. Secondly, it had no pressing need for Indian settlers, and its government clearly had no wish for a large or prosperous Indian community. Thirdly, "coolie" or indentured Indian labor was not directly an issue, as was the case in Natal, the Transvaal's southeastern neighbor.

Pillay's approach is narrow and legalistic. He relies primarily on official documents and dispatches, especially of Great Britain. This often makes for dull, tedious reading. Pillay ignores major questions and problems, and one does not get a sense of the texture of Indian activities in the Transvaal. For example, Pillay mentions "communal" and social divisions among Indians, but he fails to discuss these and their importance in Indian politics. Similarly, the division, if any, between the Indian political, social, or economic elite and the mass of the Indian populace is not mentioned. To what extent were Gandhi and other spokesmen representative of Indian feeling and interest? The internal dynamics of Indian politics in

South Africa is surely a crucial question deserving of more examination than Pillay provides.

Even within Pillay's official and legalistic framework, the book has a number of deficiencies. Pillay claims that British concern for and defense of Indian interests in the South African Republic gave way to political expediency and was sacrificed on the altar of Boer-British reconciliation after 1901. Yet the evidence provided seems to suggest that British officials were, on the contrary, sympathetic to the Republic's efforts to limit the Indian presence and that the Boer assertion that Great Britain attempted to embarrass the South African Republic on the Indian issue was essentially correct. Britain used the Indian question as one lever to advance its interests in the Transvaal; when the issue became a liability, Britain's concern for Indian rights evaporated in the face of more pressing political requirements.

Pillay's narrative is serviceable and straightforward. It provides useful information for those vitally interested in this period of South African history or Indians overseas. But his lack of a comparative or theoretical framework makes this recounting of the familiar story of the sacrifice of Indian "rights" to the interests of a local European settler community of only limited interest to others.

JOHN ZARWAN
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JOHN MCCrackEN. *Politics and Christianity in Malawi, 1875-1940: The Impact of the Livingstonia Mission in the Northern Province.* (Cambridge Commonwealth Series.) New York: Cambridge University Press. 1977. Pp. xi, 324. \$24.95.

The recent revival of interest in mission studies has had the ironic consequence of reducing the independent significance generally attributed to the missionary factor in modern African history. In this sense John McCracken's perceptive and balanced study is in the mainstream of contemporary mission history and represents a substantial, though by no means complete, re-evaluation of the Free Church of Scotland's Livingstonia Mission in Malawi. He shows, for example, how the early patterns of response to the mission depended more on the local political and economic situation than on the strength of the missionary presence. In explaining widespread "conversion," from the mid-1890s, McCracken follows Horton and others in interpreting these developments as a religious response to the wider social horizons imposed by Western intrusion. He shows also how Christianity often spread well in advance of the missionary presence and was thus a product of African rather than European activity. Even in the field of educa-

tion, where the Scottish missions have acquired such a "progressive" reputation, McCracken documents a marked decline in the standard of instruction from the 1890s through the 1930s. This was particularly the case for female students. While girls represented 22 percent of the students examined at the Overtoun Institution in 1898, their numbers had dwindled to 5 percent by 1935. "Rather than encouraging the genuine emancipation of women," concludes McCracken, "Livingstonia opened up new areas where men were supreme." In the economic arena, while Livingstonia missionaries saw themselves in the vanguard of modernization, the pressures of a southward-oriented labor migration system ensured that their activities "contributed to the emergence of a colonial slum" (p. 142) in the northern province of Malawi. Finally McCracken demonstrates that the "educated elite" produced by Livingstonia and regarded as the creators of territorial nationalism in Malawi were also deeply involved in local "native authority" courts and independent churches.

The strength of McCracken's work lies in its integration of mission history with other currents of contemporary African historiography. The relationship of the mission to the rise of nationalism is a conventional and perhaps overworked theme. But this book also connects with recent work in central African religious history in explaining both "conversion" and the development of independent churches. Likewise McCracken draws on the insights of the dependency or underdevelopment school of economic history in examining Livingstonia's economic impact in Malawi. The weakness of the book derives primarily from its almost exclusive reliance on written sources. Thus McCracken's comments on the religious interaction of African societies and the mission tend to be general, dependent on Horton's theoretical constructs, and not deeply rooted in the local situation. Nor does he get really inside the African Christian communities associated with Livingstonia, as might have been possible had he had an extensive field experience. Nonetheless McCracken's work represents a well-documented and significant contribution to the emerging literature of comparative mission history.

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ASIA AND THE EAST

ROY HOFHEINZ, JR. *The Broken Wave: The Chinese Communist Peasant Movement, 1922-1928.* Cambridge, Mass.: Harvard University Press. 1977. Pp. 355. \$16.50.

This book, contrary to its dust jacket description, is not a study of Mao Tse-tung's leadership during the early years of the Chinese Communist peasant movement. If any single individual dominates the book, it is P'eng P'ai, the father of the Chinese peasant movement. Roy Hofheinz, Jr. shows that it was P'eng, and not Mao, who led the earliest peasant revolutionary activities, served as the first principal of the Peasant Movement Institute, and first practiced the people's war. But P'eng was not alone. The book covers a large group of people; with the exception of Mao, most of them were unknown to Westerners because they did not survive the First Revolutionary Civil War. Many of their activities are told here for the first time.

The Communist peasant activities in the 1920s, especially in Kwangtung province, are meticulously researched and vividly described. The reader learns of their participation in both political struggle and military operations. Hofheinz writes in the most minute detail of the Peasant Movement Institute and the Peasant Associations. The present volume thus fills a significant gap in the history of the early Chinese Communist movement.

Though generally sympathetic toward Communists in his study, Hofheinz tries to be objective in his description and critical in his evaluation. He mentions the Communists' excessive use of violence, their association with bandits, their lack of power and even of peasant support, and their dependence on the Nationalist political and military force that ironically brought defeat to the Communist peasant movements. He also challenges the myth that people's wars are always supported by the populace by pointing out the extensive desertion from the Communist force, a phenomenon which is also evident in this reviewer's study of the Communist peasant movement during the Kiangsi period.

Thus, of the two theses—historical and analytical—that Hofheinz pursues, he has successfully achieved the first, though not without minor faults. For example, it is questionable that Ch'en Tu-hsiu, Li Ta-chao, and Mao were Nationalists before they were Communists. The extent of Russian influence over the Chinese Communist peasant work is also debatable. Hofheinz seems to contradict himself on this point. He argues that the Russians forced Chinese Communists to turn to village work and that a radical Comintern delegation urged Ch'en leftward in November 1926. But he also states that by the end of the same year, "Ch'en was persuaded by Stalin and Borodin to delay his plans for radicalizing the rural strategy" (p. 28). Contradictions may also be found with regard to Ch'en's views on the KMT-run peasant movement and his attitudes toward the Russians.

All these contradictions, however, may be factual rather than interpretive.

Hofheinz is in a more serious predicament concerning his analytical theme. His basic thesis is that rural revolution is more a political than a military struggle, and that human efforts are more essential than environmental conditions in determining the outcome of the revolution (preface). This certainly is a plausible hypothesis and there may be ample evidence to confirm its validity. But Hofheinz's study fails to substantiate it fully.

The defeat of the Autumn Harvest Insurrection, according to Hofheinz, was caused by the conditions of the day, especially the strength of the enemy. The Communists learned from their failures in Kwangning and Hua county that the success of a peasant movement required an independent military force and that they would have to win over more "purely military figures" (p. 232). In the case of Heifeng, the peasant movement was destroyed by its enemies' overwhelming armed force and Hofheinz concludes that the final outcome of rural revolution was unavoidably decided in military terms. In analyzing the peasant movement during the period as a whole he again concludes that it died of "natural causes" and that no human efforts by the leaders could have avoided defeat.

Certainly the Chinese Communists have learned lessons and gained experience from their earlier failures, and their subsequent efforts have partly created a more favorable environment. But Hofheinz's valuable study shows that the outcome of the First Revolutionary Civil War was decided more by military strength than by political appeal and that the wave of the Chinese Communist peasant movement during the 1920s was broken by the rock of circumstances rather than by human factors.

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KANG CHAO. *The Development of Cotton Textile Production in China*. With the assistance of JESSICA C. Y. CHAO. (Harvard East Asian Monographs, number 74.) Cambridge, Mass.: East Asian Research Center, Harvard University; distributed by Harvard University Press, Cambridge. 1977. Pp. xvi, 402. \$15.00.

Kang Chao has made a valuable contribution to Chinese economic history by gathering in one place hitherto scattered information on the history of cotton-making in China and by adding quantitative estimates and analytical discussions of his own. He ranges from the tenth century to the People's Republic.

The work deals with several problems of great

interest. First, why didn't the Chinese innovate in textiles like the English since they possessed all the elements of an advanced spinning technology? To explain this Chao focuses on the social organization of production. Spinning and weaving were normally carried out in rural households where female labor was available at near zero opportunity cost. Such producers clung to simple one-operator equipment and showed little inclination to specialize in a single operation. In short, China's large population, social structure, and values blocked the development of specialized, highly productive devices.

The author next turns to the spotty development of modern Chinese enterprises. Western capitalists in China, like the Chinese themselves, took a back seat to Japanese producers. Chao describes factors that gave the Japanese the ability to produce and market cottons so cheaply. The contrasting behavior of Chinese industrialists helps to explain their relatively high costs and bankruptcy rates.

Thirdly, Chao describes the well-known survival of handicraft weaving. His evidence shows that, again, extremely low labor costs permitted handicraft looms to thrive, even to export cloth, in competition with modern factories.

Finally, the author turns to the People's Republic. As an economist, he criticizes the regime on three counts. First, the prejudice that refuses to countenance family-based weaving has deprived China of an opportunity to profit from her relative advantage in cheap labor. Second, the planners have declined to import raw cotton. Since domestic farmers have been unable to supply a rapidly growing textile industry, the country has thereby missed a chance to become an important cloth exporter. Third, in an era of innovations in textile technology, China remains relatively backward. The author's estimates of per-capita cotton consumption support the observation frequently heard that the achievement of the People's Republic lies not in having created abundance but rather in equitably distributing a still scanty national product.

The book's shortcomings derive mainly from its brevity in the treatment of a large topic. For example, Chao ignores the demand side of the discussion on early technical stagnation. He does not adequately explain the speculative mentality of Republic-period industrialists or the fascinating contrast between them and Chinese entrepreneurs who were vigorously developing a putting-out system. One sadly notes that his bibliography remains far below the highest standards, in inclusiveness and in methods of citation. Despite such shortcomings, the book successfully brings old and new information together in a clear, broad

account of this important topic in economic history.

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STEPHEN ANDORS. *China's Industrial Revolution: Politics, Planning, and Management, 1949 to the Present.* (Asia Library.) New York: Pantheon Books. 1977. Pp. xviii, 344. \$17.95.

ALEXANDER ECKSTEIN. *China's Economic Revolution.* New York: Cambridge University Press. 1977. Pp. xii, 340. Cloth \$19.95, paper \$6.95.

Whatever shape its ultimate composition assumes, China's post-Mao leadership must face the problems of reinterpreting Mao Tse-tung's vision of Marxist revolutionary modernization as it meets the vexing issues that have been held in abeyance during the transition from the Mao-Chou period. The nature and extent of such reinterpretation (possibly even revisionism) will demand careful foreign observation and analysis if the next stage of the Chinese Revolution is to be properly understood, unlike the often simplistic and misleading explanations which attended earlier Maoist policy prescriptions. As the Maoist rhetoric persists in the current period of consolidation and moderation, will the salient policy changes already visible break sharply with the recent past, or will they be more subtle and continue the main elements of the Maoist strategy in somewhat modified form?

The ability not only to perceive clearly the nature of such changes but to evaluate them properly demands a comprehensive grasp of Mao's final model for revolutionary modernization. Two recent works which effectively contribute in complementary ways to a penetrating understanding of Mao's vision are the late Alexander Eckstein's *China's Economic Revolution* and Stephen Andors' *China's Industrial Revolution*. The first draws the broad strokes of China's economic development under Mao's revolutionary strategy, highlighting the processes of macro-economic structural change and quantitative outcomes; the second focuses more narrowly on the political economy of the evolution of the roles workers, technical staff, and managerial and supervisory personnel have played in running Chinese factories, relating the shape and substance of factory management to broader Maoist ideological goals and the turbulent political events attendant on the policy struggles arising from Mao's prescriptions with the varied resistances they generated.

Though both authors approach their respective intellectual tasks from different disciplines and

perspectives and write at different career stages—Eckstein from the mainstream of American academic economics at the peak of distinguished contributions to the literature on the People's Republic of China, and Andors from a younger generation of political scientists (his book was developed from his doctoral dissertation)—their complementary analyses of China's development since 1949 are founded on a parallel acceptance of the meaning of the Maoist view of revolutionary modernization. For Eckstein the Chinese "combination of power and modernity with a strong commitment to socialist values and to the spirit of self-reliance lends China its peculiar distinctiveness as a development model" (p. 280), and for Andors the differences in Chinese modernization hinge on its socialist ethos aimed at eliminating various kinds of inequality over time. For both authors egalitarianism, self-reliance, mass participation, social incentives, containment of bureaucracy, etc., help define the social environment in which modernization has been unfolding.

Eckstein's definition and assessment of China's development model, and his weighing of such problems as the needs to transform agriculture technologically, to train and motivate the scientific and technical personnel demanded by modern industry, and to meet the rising expectations for higher levels of living of China's masses follow from his careful and discerning presentation of the development process since 1949. After a valuable introduction on China's economic heritage, presented in both comparative (with Japan) and historical terms, Eckstein deals consecutively with the development strategies and policies that shaped growth immediately after 1949, the changed property relations and patterns of economic organization (leading to collectivization in agriculture, socialization in industry, and planning throughout the economy), the system of resource allocation, the quest for economic stability, the process of development and structural change, and the role foreign trade has played in development. In succinct and lucid fashion Eckstein succeeds in depicting fairly and thoroughly the process, principal characteristics, and result of Mao's vision and implementation of revolutionary modernization.

Though the title to Andors' book and his early theme statements suggest a much broader objective than he actually realizes, his focus on organization and management in Chinese factories leads the reader to many of the processes that distinguish China's industrial revolution from those that preceded it. While his context is factory management as it affects the day-to-day work environment and decision-making of workers, technicians, and supervisory personnel, Andors deftly relates the

microcosm of the work situation both to the central question of what revolutionary modernization means to the Maoists and to the struggle within the party and other mass organizations to implement Mao's experimental modes in factories.

Andors is properly concerned with such key issues as work incentives, decision-making, technical innovation, planning processes, political education, and technical and professional training whose turbulent and uneven evolution in China recapitulates the trial-and-error molding of Mao's concept of a new kind of industrial revolution. Andors' approach to these questions—the political economy of micro-management—facilitates our understanding of the rough, nonlinear struggle to fashion a radically different work environment in which "the key to revolutionization lay not simply in the organizational leadership of the party but in uniting politics with actual production operations and uniting production operations with an appropriate planning system [that] came only after years of turmoil and experimentation" (p. 245). The ideal of factory management was a collectivist work style in which participation by workers in management, and by managers in physical work, was being substituted for a hierarchic scheme of responsibility and decision-making consistent with one-man management.

The author presents his study in chronological fashion after two excellent chapters of analytic framework: one on modernization in historical perspective (Hobbes, Weber, Marx, *et al.* in the context of capitalism and developmental convergence), the other on China's historical setting (the emergence of Communism from a traditional Confucian civilization). The remaining seven chapters and conclusion provide an exciting account of the uneven road followed to revolutionary modernization in the factories as the politics of the Great Leap Forward, the Socialist Education Movement, and the Cultural Revolution and its aftermath were acted out in factory after factory with differential results. Over time the oscillations in practice led to the appearance of May 7 Cadre Schools, July 21 Colleges, three-in-one combinations, and other "new socialist things" which provided a set of institutional mechanisms reinforcing the Maoist revolutionary concept of a factory as a polity in which its citizens are to play an active role in all of its daily operations—economic, social, and political.

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GOTTFRIED-KARL KINDERMANN. *Pekings chinesische Gegenspieler: Theorie und Praxis nationalchinesischen*

Widerstandes auf Taiwan. Düsseldorf: Droste Verlag. 1977. Pp. 290. DM 34.

As the People's Republic of China's diplomatic successes of the past decade continue and the tiny circle of countries anachronistically clinging to their ties with the Taiwan regime contracts still further, the patrons and partisans of the Kuomintang cause grow ever more vocal in promoting the "Taiwan model." Gottfried-Karl Kindermann's contribution to this politicized debate is a rather insubstantial polemic that is neither brilliant nor convincing; his writing is more journalistic than scholarly. Kindermann's aim is to justify and defend the rule exercised by the self-seeking oligarchy on Taiwan rather than to explain or understand it, but he lacks the skill to do even this effectively.

This political tract is a poorly organized hodge-podge, comprising a rehash of the author's earlier writings, translations of the writings and speeches of Kuomintang ideologues from Sun Yat-sen to Chiang Ching-kuo, and interviews conducted by the author with Kuomintang bureaucrats and politicians. It repeats the standard, historically inaccurate, account of the republican struggle of the late nineteenth and early twentieth centuries, eulogizing Sun Yat-sen and grossly exaggerating his role in modern Chinese history. This historiographical version enhancing Sun's significance, as in previous instances in Chinese history, is for the purpose of legitimizing usurpation—in this case, Chiang Kai-shek's seizure of control over the Kuomintang through military might and his maintenance of that dominance through assassination, arrest, and intimidation. By distortion of facts and interpolations of falsehood, the official historiographers have concocted Chiang's rightful succession to Sun and portray Chiang's rule as the embodiment of Sun's principles. Kindermann gullibly accepts this rationalization as valid and presents Kuomintang policies in Taiwan as Sun Yat-senism in practice. He apparently lacks the sophistication to differentiate between the obvious manipulation of Sun's image for personal motives and a sincere attempt to realize the spirit of Sun's ideals.

The approach to the question of Taiwan's land reform is illustrative of the fundamental defects of the book as a whole. It is obvious that Kindermann's interest is related solely to its relevance in the debate over a non-Communist (correction, anti-Communist) road to economic development in an agrarian society. In this regard, it is interesting to note the equivocal answer given by Dr. Chiang Mon-lin, late head of the Joint Commission on Rural Reconstruction, when asked whether "land reform on Taiwan would have been carried out without American material assistance"

(p. 243). Dr. Chiang merely states that the program was a "joint effort" and avoids giving a clear answer (p. 243). Since Kindermann expresses his belief that the land reform, like other Kuomintang activities in Taiwan, represents the principles enunciated by Sun Yat-sen, he fails to ask about the very practice which so glaringly contradicts this assertion. If the Taiwan land reform is the realization of Sun Yat-senism, why is it necessary to set up a Sino-American Joint Commission to carry it out? This no more represents the intent of Sun's nationalism to eliminate China's status as a "hypo-colony" than does the police state on Taiwan approximate Sun's democracy.

LOUIS T. SIGEL

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ALVIN D. COOX. *The Anatomy of a Small War: The Soviet-Japanese Struggle for Changkufeng/Khasan, 1938.* Foreword by EDWIN O. REISCHAUER. (Contributions in Military History, number 13.) Westport, Conn.: Greenwood Press. 1977. Pp. xxvi, 409. \$25.00.

Soviet and Japanese accounts of the border warfare between their armed forces in the late 1930s differ diametrically. It has taken an American scholar, without emotional involvement or national interest in the conflict, to dissect one of the battles dispassionately. The result makes fascinating reading not only for the military historian, but for the student of international relations, because here we have a microcosm of limited wars—how they begin, develop, and are contained.

Changkufeng was a hill near Lake Khasan not far from the confluence of Manchukuo, Korea, and the USSR. In the wake of the defection of a Russian general (the director of all NKVD forces in the Soviet Far East) on June 13, 1938, Soviet security on the Siberian-Manchurian frontier was heightened. This in turn led to Japanese counter-measures. As Soviet soldiers began to string barbed wire and dig trenches west of the crest of Changkufeng (within Soviet territory according to the Russians, across the border of Manchukuo according to the Japanese), the Nineteenth Division of the Japanese army in Korea, egged on by the Kwantung Army, demanded that the Soviets withdraw from the disputed region. Japanese pressure forced the Soviets to increase their military preparations, which in turn appeared as a threat to the Japanese on the scene. Although the Japanese army, confident that dissension between the Red Army and the security forces would preclude Soviet escalation of the incident, wanted to launch an attack from the Korean bank of the Tumen River, imperial sanction was withheld. During the

ordered withdrawal of Japanese forces, a Japanese company, which had crossed the Tumen River, chanced upon several Russian soldiers south of Shachao Feng (west of Lake Khasan and to the north of Changkufeng) and decided to expel them by force. Word of fighting between Japanese and Soviet troops provided Lt. Gen. Suetaka Kamezo, the commander of the Nineteenth Division, with the opportunity to halt the Japanese withdrawal and mount an assault against Changkufeng. In bloody fighting on the evening of July 30 and at dawn on July 31, the Japanese drove the Russians out of Changkufeng and Shachao Feng respectively. Faced with a fait accompli, the headquarters of the Korean Army approved the action of the division commander.

The Soviet Union launched a counteroffensive on August 1, accompanied by air strikes. An attempt by the Korean Army to enlarge the conflict was blocked by the high command in Tokyo, which feared that a full-scale conflict with the USSR could erupt to the detriment of the China campaign; Tokyo desired a speedy termination of the incident by diplomatic negotiation. But the Soviets delayed until the score had been settled. By the time a cease-fire was negotiated on August 10 they had recaptured the southern edge of Changkufeng.

Traditional accounts, leaning on the conclusions of the war crimes tribunal, have depicted the Changkufeng incident as a planned test of Soviet strength by Japan. Alvin D. Coox, on the basis of an exhaustive study of Japanese documents and personal interviews with former Japanese officers, concludes that a number of border conflicts, including that at Changkufeng, were "accidental ignitions, fanned by misinterpretation and misunderstanding of intentions" (p. 363). Holding the Korean Army and the high command responsible for the nonretention of captured territory, the Kwantung Army failed to appreciate the resolve and ability of the USSR to fight in East Asia, and the following year escalated another dispute at Nomonhan (Khalkin Gol) at the border between Manchukuo and Outer Mongolia. The Red Army, on the other hand, had drawn the conclusion from the Changkufeng incident that the Japanese respected only force, and had made the necessary preparations to respond with overwhelming strength.

Coox's study is thoroughly documented, beautifully written, and well illustrated with attractive maps drawn by Dana Lombardy.

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ROBERT A. SCALAPINO, editor. *The Foreign Policy of Modern Japan*. Foreword by EDWIN O. REISCHAUER.

Berkeley and Los Angeles: University of California Press. 1977. Pp. xix, 426. Cloth \$21.50, paper \$5.45.

This volume results from a conference of Japanese and American scholars at Kauai, Hawaii, in January 1974. A previous binational conference in the series produced *Modern Japanese Organization and Decision-Making*, edited by Ezra Vogel (1975). Decision-making also figures extensively in the papers revised and printed here. As Edwin O. Reischauer, a participant, points out in his foreword, Japanese decisions tend to be more complex and hidden from public view than those elsewhere, and so require deeper study. In fact, these papers reveal no real consensus on the centrality of consensus, or other key concepts for the analysis of Japanese decision-making.

In addition to Reischauer's masterly introduction, there is much that may interest historians. Haruhiro Fukui focuses on "Policy-Making in the Japanese Foreign Ministry," linking the personnel and structure of the *Gaimusho* to its policies. His profile of the professional bureaucracy shows the wonted predominance of seniority and small-group decisions both before and after the Pacific War. He accurately analyzes recent friction among prewar, wartime, and postwar generations (a conflict that was anticipated in the *Gaimusho*'s American Affairs Bureau since the late sixties). Fukui compares the postwar Young Turks with junior ministry officials of the 1920s and 1930s, who opposed the drift toward militarism. Otherwise, he tends to minimize ideological differences in the ministry today.

Michael Blaker's paper, "Probe, Push, and Panic," attempts to ascertain Japan's "tactical style" of negotiating internationally. "Panic" does not seem quite the term for what he describes. In general, as the author admits, whether Japanese negotiating style is actually unique remains an "open question." The evidence does suggest a "remarkable consistency" to Japan's "bargaining behavior" (p. 101) in negotiations ranging from 1895 to 1941, and indeed, in two postwar negotiations, 1955-56 and 1970-71.

Gerald Curtis' case-study of the Tyumen oil development project shows top levels of the Tokyo government restraining private enterprise, supported by some in the *Gaimusho*, in order to separate and maintain accepted government policies toward Moscow and Peking. Curtis' analysis of the abortive Siberian scheme reinforces other recent work and his own paper in the earlier Vogel collection in reducing the overblown image of business preponderance in Tokyo's decisions.

A much fuller picture of business-government relations emerges from Chalmers Johnson's study of international economic policy as developed by

the international trade and industry ministry (MITI). While MITI's ministerial antecedents are traced to 1869, Johnson sees 1925–35 as a formative period, and goes on to show how, since its organization in 1949, MITI "... presided over the fastest growing industrial economy that ever existed ..." (p. 227). The role of consensus and similar concepts in MITI's policy decisions is carefully qualified.

Two thoughtful pieces compose a concluding summary: Seizaburo Sato of Tokyo University and Robert Scalapino, conference chairman and editor, both touch on the problems of Japanese orientation as the homeland "... moved in twelve decades from being the world's most isolated country to being the most global" (Reischauer, p. xvi). Sato finds one of the basic "foundations" of Japan's foreign policy in popular attitudes toward the world as developed through the sweep of history.

These papers present a valuable cross-national discussion of many aspects of Japanese foreign policy, mostly postwar. There is an index, not always reliable, and repeated misspellings of two Japanese names (one a place, the other personal—unfortunately that of a contributor!).

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ŌKURA SHŌ ZAISEI SHI SHITSU HEN. *Shōwa zaisei shi: Shūsen kara kōwa made*. Volume 3, *Dai san kan Amerika no taiNichi senryō seisaku* [Office of Financial History, Ministry of Finance. Financial History of the Shōwa Era: From the End of the War to Peace. Volume 3, American Policy for the Occupation of Japan]. Tokyo: Tōyō keizai shinpō sha. 1976. Pp. xcvi, 544. 7,000 yen.

Recent openings of State, Defense, and Joint Chiefs of Staff documents for the 1940s have sparked intense interest in American–East Asian policies during that critical decade. The phenomenon has not been limited to American historians. As any scholar who has visited the major archives can attest, the Japanese have been or are just about to come to the same sources. Their interest has focused on the 1945–52 period, for it was during the American Occupation that contemporary Japan was born. This book is one of the first by Japanese historians who came to maturity during the Occupation. The harbinger of a projected twenty-volume official history of postwar Japanese public finance, it grew out of a remarkable collaboration between individual scholars and the Ministry of Finance. Hata Ikuhiko, the military historian and bureaucrat who is the principal author of this volume, led a team that searched archives, interviewed surviving principals, and conducted

seminars for five years. The result of this monumental effort is the most thorough history of American policy for occupied Japan yet to appear.

Hata poses the traditional questions of Occupation historiography: he searches for its fundamental purposes; he examines those forces which reshaped policies as they were implemented; and he tries to assess the overall impact of Occupation reforms. But both his focus and methodology are unorthodox. He is more concerned with the economic than with the political aspects of Occupation policy. Unlike Western scholars who have produced excellent monographs on particular reforms, Hata addresses the whole spectrum of political and economic change. He does so with a bureaucratic political-analytical technique which yields fresh insights in each of the book's five chapters.

The first of these plunges into the byzantine labyrinth of postwar planning in Washington. Hata takes the reader down more paths than earlier writers such as Harley Notter and Hugh Burton. He places George H. Blakeslee at the center of the maze. This Clark University professor enunciated the basic goals of Occupation policy—military disarmament, political reform, and economic stabilization—which would appear in the major directives of 1945. By carefully analyzing the ideas and concerns of the many parties to the wartime debate over Japan policy, Hata demonstrates how weak the consensus behind those ends was. Given their fundamental differences of political philosophy, economic theory, and bureaucratic interest, it was hardly surprising that conflict persisted long after Americans set out to implement the policies they had defined.

These differences grew geometrically between 1945 and 1947. In chapters two and three Hata shows how varied their impact and how critical their cumulative effect could be. Bureaucratic quarrels in Washington presented leadership opportunities to those in Tokyo. General MacArthur seized many such opportunities during the first six months of the Occupation, both by imposing unplanned changes, such as control of the news media, and by waiting for Japanese officials to take the lead in implementing change. The latter, in turn, advanced views which reflected conflicting policy premises and which advanced individual career and bureaucratic interests. One result was a crazy-quilt pattern of cooperation and conflict between Americans and Japanese in implementing individual reforms. A second, painfully evident by 1948, was a fundamental contradiction between the deficit budget–inflationary mentality of Japanese officials and the price-control premises of the bureaucrats in the headquarters of the Supreme Commander for Allied Powers.

This clash produced a sense of economic crisis which, together with heightened awareness of cold-war tensions and increasing desires to shed the financial burdens of occupation, led Washington to reassess its policies. Hata carefully probes the intragovernmental debate over what to do, shattering the simplistic notion that what ensued was a "reverse course" from reform to economic recovery. On the contrary, the very differences which had contributed to the policy crisis persisted in efforts to resolve it. Former Army Undersecretary William H. Draper and Detroit banker Joseph M. Dodge, for example, both wanted to make Japan economically self-sufficient. But their views over whether to do so via currency rate manipulation and promotion of trade with Southeast Asia or by tough deflationary budgeting were never completely resolved. As Hata shows in chapters four and five, this enabled Japanese politicians to advance different policies in response to American pressures for change. The end result, blurred by the immediate impact of the Korean War, was something less than full implementation of American reform; debate persisted in Tokyo over the costs and benefits of future cooperation with Washington.

To outline Hata's arguments is only to hint at the real worth of this book. No serious student of US-Japan relations or of the Occupation can afford to overlook the statistical data and comparative policy-development charts in the text. Nor would one want to miss the guide to documentary sources, biographies of policy-makers, and chronology in the appendix. But its massive detail is also the book's greatest weakness. Whether in deference to the feelings of a principal ally, in response to the constraints imposed on an official historian, or simply in accord with the Japanese proclivity for subtlety of expression, Hata too often allows the mass of facts to obscure his arguments. Nevertheless, he shows, as no other scholar has, how complex the roots, how varied the implementation, and how mixed the results of American policy toward occupied Japan were. His emphasis on the economic dimension of that policy not only reveals new facts, but also provides the historian with another lens through which to focus on the shifting assumptions about Japanese-American relations more generally. Finally, the sophisticated analysis in this book renders questionable judgments as to the legitimacy and success or failure of American occupation policy advanced in earlier works.

This book stands as a major accomplishment of contemporary historical writing in Japan. We in the West can ignore it only at our peril.

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WERNER KELLER. *Strukturen der Unterentwicklung Indiens, 1757-1914: Eine Fallstudie über abhängige Reproduktion.* (Beiträge zur Kolonial- und Überseege-schichte, number 15.) Zurich: Atlantis. 1977. Pp. 286.

This University of Zurich doctoral dissertation is carefully crafted, and its story, that of Indian underdevelopment, is told in a tight, unpretentious way, without an inordinate number of shock effects. Werner Keller takes pains to steer away from the path of the German historical school and makes it clear that his view is that economic history should be cast in the framework of an internally consistent theory. Among possible alternatives, he selects an exposition of the famous Norwegian sociologist, Johan Galtung, about various interactions between "center" and "periphery," development initiatives from outside and from dominant inside groups, and the changes in the institutionalized interaction structure. Keller makes every effort to show that theory and data somehow "hang together"—although I am sure he would be the first to admit that this relationship is sometimes loose.

The dissertation formulates a puzzle and tries to answer it: what happened in the two hundred years which transformed a wealthy and luxurious land of the eighteenth century into the poorhouse of current times? What are the causes which changed an enlightened Indian ruling class to the present absurd bureaucracy dominated by power-crazy petty officials?

The solution starts with a description of the two-sector Moghul economy and the first phase of the British rule. Then we are told, to sum up the arguments, that a Western-controlled trade system and the British penetration of the Indian soul are the causes for the present sorrowful situation. Indian economic history, therefore should be viewed in terms of interactions between the center, "Anglostan," and the periphery, "Hindustan," with a constant transfer of resources from the latter to the former.

The dissertation, as dissertations ought to be, is probably more moving and tender than wise. First, I believe that the "relative wealth of eighteenth-century India" is probably the same myth as the "extreme poverty and misery of prerevolutionary Russia." These hypotheses come in handy as starting points to elaborate fashionable ideas. In addition, little or no credit is given to the bureaucracy of British India. The nineteenth-century English civil servants were not *déclassés*, uprooted, rootless, or alienated. They were not going to India because of ennui, *Weltschmerz*, *mal de siècle*, or Byronism. They were well-integrated young bureaucrats and army officers who were neither as-

cetics isolated from the outside world, nor the poster-type colonial masters. They liked good company, good entertainment, good books, and they did not sever the ties that bound them to their families, their milieu, and their class. Whatever their shortcomings, they saved the subcontinent from Balkanization.

What is the relation of the English language to Indian political values? Today the mass media reflect public attitudes: almost two million copies of English-language newspapers are sold daily and they cement India together. The familiar accusations of the "British yoke" are also couched in the language of Byron. The English language gave Indians a "window to the outside world" in spite of the fact that the British were primarily conquerors and exploiters of the native land. One can hardly imagine that decayed, caste-ridden, traditional India would be a better place to live if the Moghul rulers had not been conquered by the British. Are the Westerners to be blamed for the Hindu withdrawal from the physical and social world in a self-centered and mindless "spirituality" which leads to social atomism and the stupor of meditation? The problem is more complex and the conclusion that a higher GNP, a better balance of trade, and a stronger capacity to compete in the international markets would have resulted without the two centuries of Western domination, does not answer fully the puzzle of Indian underdevelopment. Keller's study is, however, well documented. In spite of the biases it is an original, well researched work.

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B. R. TOMLINSON. *The Indian National Congress and the Raj, 1929-1942: The Penultimate Phase*. Toronto: Macmillan of Canada and Maclean-Hunter Press. 1976. Pp. 208. \$25.95.

There has been an enormous range of scholarship on the subject of the Indian National Congress and its confrontation with the empire in the thirty years since independence. This literature runs the gamut from full-scale studies of the national movement as an India-wide phenomenon to a series of exceptionally fine histories of regional and local politics as a key element in the move to freedom.

B. R. Tomlinson combines the techniques of these two perceptions of nationalist history in surveying the familiar ground of imperial policy on the subcontinent and the national response of the Congress. At the same time he integrates into this large picture the political role and the deep political influence of the provinces and localities. The importance of Tomlinson's modest book is pre-

cisely this attempt at synthesis, demonstrating what the objectives and interests of each level of government and politics were, the nature of interaction within and between levels of the system, and how the implementation of new policies and programs in fact affected all of the participants and ultimately the total political system which they comprised.

Perhaps the most dramatic policy change was the Government of India Act of 1935 designed to devolve political participation and in the process to raise new and necessary revenues. It was also presumed that such constitutional changes would attract Indian supporters to the Raj, thereby achieving the other critical object, namely, political tranquility. But as happened so often in British India, the best official intentions did not inevitably bring forth anticipated results. As it developed, the Government of India Act of 1935, by introducing provincial autonomy, opened up a vast new reservoir of political opportunity in India with the result that the Congress, which controlled six of the eleven provincial ministries in the 1936-37 elections, came to a dominant political position from which it was to achieve the ultimate objective of freedom, and which it would maintain for nearly forty years until the remarkable events of 1977. The thrust of Tomlinson's argument is this fact of Congress dominance and the "provincialization" of Congress politics as it came to reflect the pressures and interests of the localities and regions in which it operated. What Tomlinson does well is provide a clear exposition of the rise and nature of the Indian National Congress as movement and party against the backdrop of a declining imperial power. The major contribution of the book is the statement that this is a provincial as well as a national phenomenon with implications far beyond 1947. He concludes, quite rightly, that "the origins of the political system of Independent India must be sought in the events of 1934-39" (p. 158).

The author's regional emphasis would have been considerably more effective had he gone to greater pains to identify some of his provincial actors. Unless one is intimately familiar with Bihar, U. P., Orissa, and C. P. which are the provinces Tomlinson focusses on, even prominent names like Swami Sahajanand, Sri Krishna Singh, G. B. Pant, R. A. Kidwai, Ravi Shankar Shukla and others will be meaningless. There is a certain two-dimensional quality about simply naming political names as authors of factions and participants in patronage games without providing other social or ideological associations.

It is perhaps germane to say something about the use of vernacular materials in a study of this kind. It is a striking and happy fact for all of us that in British India even provincial politicians

were often communicating in English. Tomlinson's research demonstrates how effectively serious scholarship can be executed down to the regional level by relying almost exclusively on English language sources. It is also the case, however, that there is an enormous corpus of vernacular sources available to the historian of provincial and local movements. These materials need to be utilized, ultimately, because they are there. In addition, because they exist in the language of all of the people, including the English-speaking cadres and leaders, they may conceivably reveal indigenous conceptions of political behavior not readily apparent in English sources interpreted by speakers of English. In either case they will provide scholars new opportunities of penetrating the political system with yet more enhanced sophistication and refinement.

WALTER HAUSER
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A. JEYARATNAM WILSON. *Electoral Politics in an Emergent State: The Ceylon General Election of May, 1970*. (Perspectives on Development, number 3.) New York: Cambridge University Press. 1975. Pp. xv, 240. \$21.00.

Hindsight shows that the general election in Sri Lanka (Ceylon) of May 1970 was not the landmark political event—despite the shoring up of Mrs. Sirimavo Bandaranaike's leadership of various socialist elements into a United Front—that the author believed it to be when he completed this book in 1973. A more recent election (1977) has returned the United National Party (UNP) to power. Indeed, the new UNP head, J. R. Jayawardene, has introduced legislation that would change Sri Lanka's constitution to a presidential order somewhat similar to France's Fifth Republic, with consequences that will change the electoral strategies of the country's political parties. The regularities and general fairness of elections in Sri Lanka since independence do not inform us adequately about the state's political system, any more than elections in Malaysia or India do over much the same period. The major crises in all three countries in the 1970s have arisen more from internal economic, social, and ethnic pressures, as well as from international influences, than from opportunities to vote.

A. Jeyaratnam Wilson is an experienced political scientist from Sri Lanka. His study of the 1970 election is thorough, if somewhat old fashioned in its methodology. Wilson states in his introduction that his method follows the Nuffield College election study model. Nevertheless, the absence of survey research of the voters, both before and after the

election, reduces the quality of his analysis concerning political trends in Sri Lanka. The strength of his contribution rests more on the description of the election, including considerable attention to election manifestos, the analysis of election results as shares from aggregated data, and an outlining of contending party positions over selected national policy issues. The several socialist and communist parties are delineated clearly, as is the dilemma of the Tamil-speaking minority located especially in the northern sector of the island. The influence of the newspapers is given more attention than in most such stories, and to good advantage. Because of the small size of Sri Lanka and relatively high literacy rates, newspapers and other printed literature can have great impact, and apparently did in this election.

The 1970 general election was not a landmark event, nor is this book for the literature on electoral behavior. Both, nonetheless, constitute useful contributions in exhibiting particular aspects of the political development currently taking place in Sri Lanka.

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JOHN MCCARTHY. *Australia and Imperial Defence, 1918-39: A Study in Air and Sea Power*. St. Lucia: University of Queensland Press. 1976. Pp. 227. \$9.95 (Australian).

John McCarthy's *Australia and Imperial Defence, 1918-39* is a pioneering contribution to a little-known and poorly understood area of twentieth-century Australian history. It is a very worthwhile administrative-strategic study of Australian defense policy between the wars and a great improvement on the only existing accounts which are to be found in the background chapters of the official history volumes of *Australia in the War, 1939-1945*. Drawing on the British and Australian government archives as well as a wide range of supplementary sources, McCarthy traces out the Royal Australian Air Force's struggle to establish itself against the context of an imperial strategy which assumed that the Royal Navy could guarantee the security of all British interests in the Far East. It is a story of British deceit and obfuscation, Australian confusion and mismanagement, and, not least of all, of interservice rivalry and intrigue.

In the author's view, the chief obstacle to the development of the R.A.A.F. in these years was the belief that Australian safety was dependent on the creation of a major naval base at Singapore. The Australian prime minister, Stanley Bruce, had accepted the proposition at the 1923 imperial

conference, and throughout the 1920s and 1930s this doctrine remained the cornerstone of Australia's defense thinking. In this framework it was difficult for the R.A.A.F. to justify itself along strategic lines. Wing Commander Richard Williams, the first Chief of the Air Staff, rarely attempted to press his case on such grounds but rather looked to the British Air Ministry for support against the contending claims of the Naval Board, which accepted the Singapore strategy, and the Military Board, which stressed the need to prepare a local defense against invasion.

McCarthy is critical of the Australian authorities for so naively accepting the identity of British and Australian interests and for relying so blindly on the British navy and the Singapore base. In the most interesting and original chapter of the book he shows how after 1934, when the Australian government placed orders in the United Kingdom for new aircraft, the British failed to meet Australia's needs because of their own rearmament program and because of the higher priority accorded Turkey and Finland; when the Australian government attempted to manufacture its own aircraft with the cooperation of North American Aviation—an initiative which resulted in the building of the Wirraway—Britain put every possible obstacle in their path.

Undoubtedly Australia's political leaders were too prone to take British assurances at face value and to let imperial sentiment and political expediency get the better of their judgment. Nevertheless, perhaps because his sources are primarily naval and air rather than military and diplomatic, or perhaps because he allows the fact of World War Two to dominate his historical perspective, McCarthy tends to be too severe in his strictures. It would appear more reasonable than he allows for Australia in the 1920s to have relied upon the British navy and to have adopted a low defense profile. Japan, the only possible aggressor, had forsaken its imperial ambitions and, under a Liberal civil government, had fulfilled the agreements made at the Washington Conference to restrict naval building and fortifications and to respect Chinese integrity. Moreover, Britain, secure from the fear of a European threat, was free to deploy its fleet in the Pacific. It was unfortunate for the R.A.A.F. that it should come into being at a time when Australia's interest in defense and concern for security was at a low ebb.

After the rise of Nazism in Europe and the resurgence of Japanese imperialism in the Far East, the Australian government recognized that the British Empire might be faced with a war on two fronts, and that in such an eventuality, the Singapore strategy might be found wanting. McCarthy points out that Prime Minister Joseph Lyons—

even if unavailing—urged the appeasement of Japan in Manchuria after 1935 as a basis for a *modus vivendi* and sought British and American support for a Pacific pact under which all signatories would promise to respect the status quo in the region. In 1934 the Australian government began a rearmament program which, in contrast to the 1920s, gave the lion's share of the additional defense budget to the army and air force, that is, to local defense.

When all this is said, however, the substance of the criticism remains. The measures were half-hearted; the results were meager. Australia's leaders in the 1930s lacked the clear perception of danger and the determined purpose to act which had led their predecessors in the years before 1914 to introduce universal military training and to build up the Royal Australian Navy. The explanation for this failure is not to be found merely in the Australian government's deference to the mother country or its simpleminded dependence on the Royal Navy. Of at least equal importance were the domestic divisions which had their roots in the conscription crises of 1916 and 1917; the bitterness of that period colored and conditioned the politics of a generation.

NEVILLE MEANEY
University of Sydney

UNITED STATES

L. S. CRESSMAN. *Prehistory of the Far West: Homes of Vanished Peoples*. Salt Lake City: University of Utah Press. 1977. Pp. 248. \$15.00.

Luther Cressman has been involved in studying the prehistory of northwestern North America, especially Oregon, for nearly fifty years. Few other areas of the North American continent have been treated to such a continuous, intensive scrutiny. During this time Cressman pioneered in many techniques and cross-disciplinary investigations. This book, in Cressman's synthesis entitled "Time and the Land," gives a complete, yet general, discussion of glacial geology and the effect of the glaciers on continental topography and climate.

There is still a diversity of opinion among prehistorians as to the time when the first ancestors of the American Indians entered the New World. Cressman attacks this problem in his chapter entitled "The Wanderers." Some investigators will not accept parts of the data Cressman cites as being well enough documented to be useful. He is convinced of an early date for man's entry into the New World perhaps before 35,000 years ago.

Following this are discussions of both the biological and linguistic character of the Indians of

Western North America, showing the great diversity and variations of those people. These data are used by Cressman in confirming his interpretation that a long time range is needed to understand the presence of man in the New World.

Many archeological sites that have been examined by prehistorians from California to British Columbia are described in a useful summary. This is the only place I know of where such a summary of western North America exists. It will certainly be useful to specialists but is also presented in such a way that the more general reader will benefit.

The entire book is well written without the use of overly technical language. Cressman's deep intellectual and emotional involvement with the broad subject shines through. His technical grasp of the subject is unsurpassed. Parts of the argument are somewhat controversial, but Cressman's marshaling of the evidence puts these in perspective. This book is to be highly recommended, to the specialist in prehistory, to those interested in the geology of the North American continent, and to those who are enthralled by one person's desire to learn and tell others of his results.

MELVIN L. FOWLER
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WILLIAM C. HAVARD and JOSEPH L. BERND, editors.
200 Years of the Republic in Retrospect. Charlottesville:
University Press of Virginia. 1976. Pp. xi, 348.
\$12.50.

Planned to illustrate continuity over two hundred years, calculated to contribute to the maintenance of the constitutional system, this book of essays merits serious consideration because of the inclusion of some very stimulating work. Predictably most of the essays offer little of value to the scholar, even as they inform the general reader in a comprehensive way about a variety of topics. But the good ones make the reading worthwhile. This review concentrates on these essays.

Vincent Ostrom's comparative analysis of the theory of constitutional choice suffers from one major flaw. He has defined the problem cogently, and has illustrated two historical solutions in the works and thought of Hobbes and Lenin. The attempt to define an "American" solution fails to persuade, however, because Ostrom has not grounded this part of the analysis in the works and thought of historic, seminal, American thinkers. As a result, the reader wonders whether Ostrom's theory has the assumed relationship to an operational American theory.

Dean Burnham argues that the American political system is in a crisis stage. In the past, periodic—virtually generational—critical realign-

ments have allowed subtle alternations in the meaning and significance of terms so that the political system developed in consonance with social and economic conditions. Yet the contemporary situation makes manifest anomalies in such number and of sufficient importance to augur fundamental change in the political system itself. Whether one agrees or disagrees, one comes away from the discussion convinced of the seriousness of the current crisis in political affairs.

Three other essays deserve brief mention. Robert Harris carefully explores the differences and similarities between the "old" judicial review, with its focus on property rights, and the "new," with the emphasis on individual rights. In either case, judicial review becomes judicial supremacy. Numan Bartley offers new insights into the South and sectionalism in American history. The critical factor in explaining sectionalism of whatever form, according to Bartley, has been ethnicity. Finally, Louis Gerson asks why ethnic concerns surface periodically in American society, and argues persuasively that acculturation and assimilation, as in the past, will continue to characterize the condition of newcomers in this "nation of immigrants."

Certainly some of the remaining essays sound provocative themes. Louis Simpson's thesis that the "Republic of Letters" emerged stillborn in the revolutionary era serves in great part to illuminate the triumph of transcendentalism. George Carey and James McClellan appear almost to shout in the wilderness, so alien are their arguments to most of the other essays. The contentions that the Declaration of Independence must be read in its entirety for understanding, and that the American tradition did not attain maturity until after the adoption of the Constitution, are beyond dispute, although not necessarily determinative of the conclusions Carey and McClellan suggest.

Books of essays like this one rarely achieve their stated purposes, since the purposes are usually all-encompassing and vague. But if every such book included a few gems of the quality of the good essays here, readers could hardly complain.

GEORGE M. DENNISON
Colorado State University

ROBERT T. HANDY. *A History of the Churches in the United States and Canada*. (Oxford History of the Christian Church.) New York: Oxford University Press. 1977. Pp. ix, 471. \$19.95.

Though the landscape is dotted with many books on religion in America, this impressive volume makes a special sort of mark. As its title suggests, the scope of Robert T. Handy's latest work is at

once broader and narrower than many predecessors in the field. Its breadth is explicit: everything from the Rio Grande north. American historians do not totally ignore things Canadian; yet the reverberations of Queen Anne's War, of the Seven Years' War, and of the Quebec Act are followed in detail only within "the" thirteen colonies. How refreshing, therefore, to be able to observe these and other events as they shaped Canadian church history. Most American students will learn the Canadian story only from books such as this, since Canadian history continues to be widely and stubbornly ignored in departments of history across the United States. Handy's work has also a subtler breadth hinted at by its inclusion in the Oxford History of the Christian Church. Not only is the book addressed to a wider English audience, but the linear perspective is as much that of twenty centuries of Christian history as it is five centuries of North American history.

On the other hand, the title also narrows the scope to "the churches." Conceived largely within the confines of Christianity (some attention is given to Judaism), Handy's history is heavily institutional and denominational, which is not to say that he returns to some older and more parochial form of church history. Well aware of, and sensitive to, the social, political, and cultural currents of national and international life, Handy is no mere company chronicler. Yet, the "churches" unambiguously and understandably constitute his central focus.

Twelve chapters carry the story from Europe's early penetration of the New World down to the complexities of renewal and decline in the 1970s. Three chapters are devoted exclusively to Canadian developments, while in three others Canadian and American church history can be conveniently combined. The remaining six chapters, which center on ecclesiastical affairs in the United States suggest no imbalance if one recalls that Canadian population is about one-tenth that of the more southerly nation. And while the two national histories have some obvious parallels in industrial challenges, territorial expansion, urban growth, intellectual milieu, and world wars, the author does not force a common grid upon them. Of the period from 1800 to 1867, Handy observes, "there was a much greater emphasis on continuity with the past in Canadian than in American religion, less experimentation, less willingness to take fresh starts and make new departures in religion. . . . Canada was a rather 'churchly' nation in 1867 with the overwhelming majority of the population claimed as members or constituents of its denominations" (p. 259).

Handy notes other significant differences in fact and in tone. Americans have a tendency to invest

religious feelings in their nation while Canadian religious feeling remains "much more within church boundaries" (p. 260). The two nations have quite different understandings of church-state separation, reflected, for example, in contrasting systems of higher education and in the long controversy over the Clergy Reserves. The unmistakable Gallic flavor of Canada's Roman Catholicism distinguishes it from Catholicism to the South. And the denominational spectrum in Canada is much narrower, consisting principally of Catholicism, Anglicanism, and the United Church (the latter being formed in 1925 of a large Methodist bloc, a small Congregational one, and the majority of a badly divided Presbyterian following). Over a century after 1867, Canada was still more "churchly" than the United States, both in the sense of having a larger percentage of its population as church members and in the sense of having the vast majority (more than 75 percent) within these three churches alone.

The strengths of this book are many. Its organization is imaginative and instructive, as when, for example, Anglo-Catholicism, Mercersburg theology and Baptist Landmarkism are grouped together as "high church movements." Reliability is assured, in part by the author's own earlier solid research, and in part by his discriminating use of first-rate sources. Documentation is well done, but not overdone; the twenty-one page bibliography guides wisely and without pomp. Even the index deserves a word of praise since the print does not demand bionic vision.

This first volume to appear in a projected twenty-volume Oxford History of the Christian Church does what good history is supposed to do: it brings new dimensions in self understanding. But it goes even a second mile, helping us also to understand our neighbor—perhaps an altogether appropriate task for Christian history to perform.

EDWIN S. GAUSTAD
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ROBERT K. ACKERMAN. *South Carolina Colonial Land Policies*. (Tricentennial Studies, number 9.) Columbia: University of South Carolina Press for South Carolina Tricentennial Commission. 1977. Pp. 133. \$14.95.

MAURICE A. CROUSE. *The Public Treasury of Colonial South Carolina*. (Tricentennial Studies, number 10.) Columbia: University of South Carolina Press for South Carolina Tricentennial Commission. 1977. Pp. xiii, 142. \$14.95.

One simply can no longer write the history of colonial America from the collections of one ar-

chive, no matter how comfortably situated. The pursuit of any individual, any institution, or any idea will rarely if ever begin and end at the door of the same repository. Perhaps there was once a time when convenience or expense served as arguments for not venturing beyond what lay closest at hand, but the days of cheap travel and microfilm have been with us for a while. Historians should be expected to discover and to use all of the materials that are germane to their topics.

Here for review are two books that fail badly by that criterion and with unhappy results not only for the authors, who have written poorer books than they might have otherwise, but also for other scholars working in the same area and for colonial historians in general. With due regard for the impressive collections of the South Carolina Department of Archives and History, one cannot write the history of the colony without looking beyond Columbia. To take an obvious example, no one believes that Noel Sainsbury in the 1890s copied into thirty-six volumes everything in the Public Record Office dealing with South Carolina. M. Eugene Sirmans in his book on *Colonial South Carolina* (1966, p. 361) notes that even for those volumes in the Colonial Office series that Sainsbury did transcribe he left out things. The items he failed to copy were largely enclosures in the letters from the governors to the Board of Trade, and these enclosures most often were items such as the reports of the colonial treasurers, tax or duty accounts, the reports of the receiver general of the quit rents, and the like. Anyone who has looked at Charles M. Andrews, *Guide to the Materials for American History, to 1783, in the Public Record Office of Great Britain* (1912-14, 1: 115-18) knows that there are whole volumes in the Colonial Office series that Sainsbury did not even begin to transcribe (including C.O. 5/512 containing reports of the colonial treasurer and the receiver general of the quit rents). Those adventurous enough to look beyond these four pages in Andrews will discover whole series of papers outside the Colonial Office records with material for South Carolina (including the Treasury Papers which appear to have a good deal that is potentially relevant to these two studies). And as Andrews himself admits, his *Guide* is only a partial introduction to the PRO. But both Robert Ackerman and Maurice Crouse rely exclusively on the Sainsbury Transcripts in the South Carolina Archives for PRO materials. How can one begin to discuss the ideas and arguments in these books when the basic research is so flawed as to cast immediate doubt on anything the authors say?

Nor need we venture across the Atlantic Ocean to begin a search for potentially useful primary sources or even secondary works that these authors show no sign of being informed by. Crouse does

not appear to know that Philip M. Hamer, George C. Rogers, Jr., and others have been at work publishing *The Papers of Henry Laurens* (1968 to date) for over fifteen years now. Ackerman gives no indication of having read Alan D. Watson's fine dissertation on "The Quitrent System in Royal South Carolina" (University of South Carolina, 1971). Ironically, of course, either man could have repaired these omissions quite conveniently right in Columbia!

The authors have signed their names to works which I am sure are not their best efforts. Others working in related subjects are cheated by the publication of such poorly researched and, in the case of Crouse, poorly thought-out books. Consider for a moment the likelihood of Alan Watson finding in this market a publisher for his study of the same subject as Ackerman's book. Will the University of South Carolina Press publish it too? The whole of the historical profession is diminished by the publication of "books" such as these.

JOHN J. MCCUSKER
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College Park*

DAVID FREEMAN HAWKE. *Franklin*. New York: Harper and Row. 1976. Pp. ix, 436. \$15.95.

David Freeman Hawke has given us useful studies of Benjamin Rush and Thomas Paine. His textbook, *The Colonial Experience*, is widely respected. But Hawke's *Franklin* is not likely to delight his admirers.

Hawke's original intention was a full-length portrait with a focus on the Franklin character, a character he describes as "one of the most elusive produced by eighteenth-century America." So elusive, in fact, that Hawke leaves his readers to make their own judgment. He also leaves Franklin rather abruptly—in July 1776.

The original working title for this book was "Benjamin Franklin—What Manner of Man?" A current question is "What Manner of Book?" *Franklin* is not a full biography in the sense that no second volume can be anticipated; nor is it another exercise in psychohistory (for which this reviewer is grateful). Hawke seems genuinely undecided as to his purpose, apparently alternately fascinated and overwhelmed by his subject. But he has given us glimpses of Franklin that suggest the kind of study this might have been. For example, there is a delightful snapshot of Franklin in attendance at the Second Continental Congress in the summer of 1775: "Now here he sat, 'a short, fat, trunched old man in plain dress, bald pate, and short white locks. And he sat saying nothing, in *expressive silence*.'"

There is also much that is familiar: Hawke's treatment of Franklin's courtship of Thomas Godfrey's daughter (when Franklin invited the parents to mortgage their home to raise a dowry, with which he could discharge his debts), Franklin's common-law marriage to Deborah Read (who could never pass a writing competency test), and, of course, Franklin's electrical experiments. Franklin the scientist does not seem to have much fascination for Hawke, but he does offer an interesting discussion of Franklin's kite-flying. Hawke speculates that Franklin may have had his Negro slave actually fly the kite, in case of another electric shock, and so avoided claiming more than the execution of the experiment.

It is Franklin the intellectual who is particularly neglected in this study. Although Hawke provides a good (if terse) account of Franklin founding the Library Company of Philadelphia, he fails to discuss either Franklin's influence over the institution or its later significance for the American Revolution. Again, Franklin's personal library is mentioned, but its scale, its contents, the special volumes with Franklin's marginalia, are ignored. What did Franklin read? What, if anything, did he gain from such reading? Why was his acquaintance with Lord Kames so notable? Indeed, was Kames anything more than "Scotland's most eminent jurist"? And why do we learn so little about Franklin's British friends—such as James Burgh, who goes unmentioned? Such omissions are perplexing.

Of course Franklin is a challenge. He lived a long and active life. The quantity of published and unpublished materials must daunt any honest potential biographer. Hawke reached the point where he felt "overwhelmed by the sea of paper." Franklin became to Hawke what John Adams was to the late Catherine Drinker Bowen: she did not complete her biography of Adams but concluded with her treatment of Adams and the final decision for independence in 1776. Still, she wrote with a lively enthusiasm and insight readers of *Franklin* will miss.

TREVOR COLBOURN
San Diego State University

JOHN ZVESPER. *Political Philosophy and Rhetoric: A Study of the Origins of American Party Politics*. (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press. 1977. Pp. vii, 237. \$16.95.

More than any other subject, early national historians have been preoccupied with American political party origins and development. Theories have

been proffered which range over a broad spectrum of causes—ideological, psychological, geographical, international, accidental, and of course, economic. Depending upon the definition of the term "party," some scholars trace party origins to the period before the Constitution; others deny their existence until well into the nineteenth century. John Zvesper's small volume is meant neither to synthesize nor to resolve these historical disputes. The ethnic dimension of American political behavior is not mentioned. He does not enter the fray between anti-Beard and post-Beard enthusiasts. The disparity between congressional issues and local concerns is ignored. Zvesper is familiar with the vast literature, including the quantifying studies of legislative alignments, all of which he cites. (There are forty-eight pages of footnotes). But he takes a different tack, an old-fashioned one really, in attempting to trace party behavior in the 1790s. He concentrates upon the fundamental differences in philosophic assumption which divided Federalists and Republicans; upon the inevitable gap which developed between these philosophies and their practical application; and upon the "tension" which occurred between party aims and "the rhetorical necessities" (p. 15). The first two themes follow well-worn paths; the last adds a dimension which makes Zvesper's book a welcome addition to scholarship. To Zvesper, rhetoric is at times more significant than substance, and particular political parties have been unusually adept at it. If Americans have made politics a religion, rhetoric is its theological expression.

One wishes that the author had explained more precisely what he means by rhetoric, as, for example, Gordon Wood does in his essay, "Rhetoric and Reality in the American Revolution." When Zvesper comments, in a parenthetical remark, that "Republicans labored under a logical rather than a rhetorical weakness; in fact, it was a rhetorical strength—rhetoric being, as Aristotle says, the antistrophe of dialectic" (p. 32), the puzzlement of many readers will not be removed by scurrying to the dictionary. *The Federalist* is seen as rhetoric; so are pamphlets, speeches in Congress, newspaper essays, and even personal letters. Is rhetoric any statement made to persuade? One wonders what it is, and what it is not. However rhetoric is defined, Zvesper is right in maintaining that Republican idealism was better suited than Federalist realism to rhetorical appeals and strategies. Indeed, the main strength of his book is the proof of this point, beginning with the election of 1792 and culminating with Jefferson's victory in 1800. After that date, writes Zvesper, while it would be "unfair to limit the causes of the decline of the Federalist party to rhetorical short-comings . . .," it must "be consid-

ered as one of the most important causes of the party's undoing" (p. 183).

MORTON BORDEN
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Santa Barbara*

JOHN T. NOONAN, JR. *The Antelope: The Ordeal of the Recaptured Africans in the Administrations of James Monroe and John Quincy Adams*. Berkeley and Los Angeles: University of California Press. 1977. Pp. vii, 198. \$10.00.

This slender volume is a case study of the fate of 281 slaves captured aboard the *Antelope* by a U.S. Treasury cutter off the coast of Florida on June 29, 1820. The *Antelope* was an American-built ship, registered in Spain, licensed by the Royal Governor of Cuba to engage in the slave trade, but flying the flag of José Artigas, the founding father of Uruguay, who was at war with Spain, Portugal, and another revolutionary government in Buenos Aires.

At least three federal laws applied: the law prohibiting the African slave trade, the Neutrality Act which made it a federal crime to outfit Latin American privateers in U.S. ports, and an act passed to aid the Colonization Society which authorized the president of the United States to see to the "safe-keeping, support, and removal beyond the United States" of Africans captured in the slave trade. On the basis of these laws, claimants argued for Portuguese and Spanish ownership, as well as for freeing the slaves to be sent back to Africa. A final disposition, based upon a lottery, divided the still surviving slaves between Spanish claimants and Liberia. The complex legal maneuvering, which included three Supreme Court appeals and a private bill designed to validate the purchase of slaves awarded to the Spanish by a Georgia congressman, took eight years to complete.

The author, John T. Noonan, Jr., teaches law at the University of California at Berkeley and is not by training a historian. He is well equipped to trace the twisted and obscured journey of the *Antelope* insofar as legal issues are concerned. He has done this mapping, however, with the sensibilities of a historian and without the vices commonly associated with legal writing. Although his focus is upon the legal issues, Noonan also provides a short, but adequate, introduction to the subject and a retrospect which provides a backward look at the case a decade later. He makes a conscious attempt to place the decisions of the courts and the administrations in their historic settings, explaining the often more pressing issues occupying the attention of judges, secretaries of states, and presi-

dents. Above all, he writes clearly, taking pains to explain the legal terms and procedures of the day carefully and simply. The book is a useful microscopic study of a formative period in American domestic and international jurisprudence.

DWIGHT HOOVER
Ball State University

JEAN H. BAKER. *Ambivalent Americans: The Know-Nothing Party in Maryland*. Baltimore: Johns Hopkins University Press. 1977. Pp. xvii, 206. \$14.00.

Ever since the McCarthy era, students of American politics have been intrigued by the underlying strain of right-wing paranoia endemic to the political system. Richard Hofstadter riveted attention on the Populists of the 1890s and their stepchildren of the 1930s. Then Frank L. Klement placed the Civil War Copperheads in this tradition. Now the Know-Nothings have moved to center stage with the research of Michael Holt, David Brion Davis, and Jean H. Baker. Baker's book on the Maryland Know-Nothings is the first thorough analysis of this puzzling group as a political movement.

Unlike Holt and the other "paranoid style" scholars, Baker rejects the irrationality stereotype and portrays the Know-Nothings as "ambivalent reformers" in the Mugwump-Progressive tradition who sought to "purify" America through political processes. Know-Nothings, Baker avers, were not an aberration in liberal consensual American politics but a reflection of the historic conflict between nativists and ethnics concerning the nature of American society. In contrast to those who ascribe Know-Nothingism to the temporary socioeconomic descriptions of the 1850s, Baker explains that the American movement emerged in the prosperous 1840s as a response by native Americans to heavy Catholic immigration (p. xii). Baker further concludes that nativism, rather than paranoiac antipartyism, caused the demise of the Whig Party (p. 154).

Baker employs both quantitative and qualitative methods to analyze the six aspects of political parties: their setting, ideology, leaders, followers, organizational structure, and legislative behavior. The setting in Maryland was the rapid economic growth, heavy immigration, and institutional dislocations of the 1830s and 1840s that spawned nativism. The response was an intense but rational crusade "to return a changing America into a sustaining America" (p. 30). Baker measured the level of intensity in American party politics by a comparative content analysis of rhetoric in American and Democratic party platforms. Americans were less rhetorical than Democrats, they addressed issues more specifically, stated future pol-

icy goals clearly, and, above all, they muted their anti-Catholic tirades.

To explore other behavioral facets of the American movement, Baker prepared a collective biographical portrait of one hundred and ten Know-Nothing leaders (who are compared to a like number of Democrat leaders), made a scaling and cluster bloc analysis of American Party law-makers in the Maryland legislature, and carried out simple and multiple correlation analyses of county-level popular voting data. Each of these procedures entailed much dogged research in various serial sources and extensive computer "number-crunching," but the results are not maximized as a result of methodological inadequacies. In the collective biographical study, no statistical measures with significance levels are employed to compare the American and Democrat leaders in terms of wealth, age, occupation, and slaveholding. Similarly, the legislative scaling analysis is marred by the author's nonsystematic method of selecting votes for inclusion. Given the long-standard Guttman scaling technique, one wonders why Baker chose to follow the unique procedure of her mentor, David Donald, in *The Politics of Reconstruction* (1963). Additionally, the multiple regression analysis of voting behavior, based on twenty-two variables, is flawed by the failure to check for multicollinearity and to use a step-wise procedure to determine which of the numerous variables contributes more to the outcome.

Baker seems to have braced herself for such methodological criticism. The concluding portion of the book's introduction offers a rationalization for employing "elementary quantitative tools." For this reviewer at least, the author's sophomoric caveats against the new political history—that it is boring, jargon-laden, ignorant of traditional sources, assumes that "the best evidence is the most countable," employs models as a mold, requires a "lifetime of research efforts to employ" (pp. xv-xvi, 62, 164, 167, *passim*)—are an unwarranted intrusion in a very readable and persuasive book. The author's remarkable ability "to turn a phrase" and the thorough mastery of all aspects of her topic is more than sufficient to commend this book to scholars.

ROBERT P. SWIERENGA
Kent State University

DONALD G. MATHEWS. *Religion in the Old South*. (Chicago History of American Religion.) Chicago: University of Chicago Press. 1977. Pp. xx, 274. \$10.95.

Although this book is a volume in the Chicago History of American Religion series, it is not a

history of religion in the Old South. Donald Mathews describes it as "an invitation to further discussion of the character, functions, and significance of religion in shaping and defining the South" (pp. xiii-xiv). His aim is to "suggest themes, structures, and ideas of the Southern Evangelical tradition which enabled people to come to terms with their social existence" (p. 186).

The most influential aspect of religion in the South was evangelicalism, a term whose use by the author "will annoy those who like clean-cut and precise definitions" (p. xvi). Evangelicalism is described as being "a social process as well as a religious perception" (p. xvii). It is the social process, which was never stable but ever changing from one generation to the next, and the effect which this process had upon the religious values and perception of southerners, that is the focus of the author's attention.

Evangelicalism, as social process, entered the South in the mid-eighteenth century and provided the vehicle whereby its converts were given confidence and courage to reject the life-style and values of the elites and to create another value system and a new mode of social organization (pp. 14, 37-38). The nineteenth century witnessed "a much different process;" it was less "volatile, alienated, defiant, and charismatic" than it had been in the previous century. The evangelicals had become the elites, and the process now was to provide "symbols, style of self-control, and rules of social decorum" (p. 83) which would become the norms for society.

Evangelicalism had a profound influence upon women and blacks. For women it brought them "together in churches, academies . . . and societies" and provided "the framework within which to form [a] homosocial network" (p. 110). Although "black religion . . . was a churning suspension of ideas and behavior patterns fed by African and Christian traditions" (p. 208), "the similarity of Evangelicalism to highly valued aspects of African tradition . . . helped convert Africans to Christianity" (p. 196). These converts, unhampered by racism or ethnocentrism or status consciousness, achieved a degree of religious maturity which was denied white Christians (pp. 185-86). Christianity strengthened the blacks "self-esteem," and its message of forgiveness and forbearance enabled the slave "to make the oppressor and his instruments psychologically irrelevant" (pp. 220, 229). In the conversion experience blacks "found psychological and ideological resources" which enabled them to endure a situation from which they "would one day be freed" (p. 243).

This study, which attempts to utilize methods and concepts from sociology and psychology, offers a new dimension to the discussion of religion

in the Old South. If it constitutes the "first word" (p. xiii) in such an analysis the final summation will be at a distant date. "Each church, society, and congregational record" in the region will first have to be "analyzed as evidence of a process of social interaction and projection of self into a public personality" (p. 268). Although the social process formula could probably be used to explain the value system of any society at any given time, it is hardly convincing. Perhaps this reviewer is one of those annoyed by the imprecise and amorphous nature of the thesis or maybe he was somewhat turned off by what seemed to be an excessive use of jargon. The thesis is stimulating and this study should elicit further inquiry and discussion of a fascinating subject.

W. HARRISON DANIEL
University of Richmond

RAY HOLDER. *William Winans: Methodist Leader in Antebellum Mississippi*. Jackson: University Press of Mississippi. 1977. Pp. viii, 232. \$7.95.

William Winans was one of those eccentric Methodist pioneers who make the history of early American Methodism so colorful. Self-educated, zealous to the point of near fanaticism, austere, alternately humble and self-righteous, Winans was the leader of the Methodist Church in the Old Southwest for forty-seven years. Neither distance, weather, nor personal hardship prevented him from preaching, attending conference, raising funds for Centenary College, or participating in camp meetings. In such activities he was the quintessential itinerant, the stuff of which Methodist legend is made. Yet the argumentative Winans was more; a slaveholder by marriage, he came to identify completely with his adopted region. While he abhorred slavery in the abstract and was a fervent supporter of the American Colonization Society, he nevertheless felt southern Christians in a slaveholding society had a duty to be slaveholders. On the floor of the 1836 General Conference he called himself a slaveholder "on principle" (p. 148). He was a strong advocate of the formation of the Methodist Episcopal Church, South, convinced that abolitionist sentiment in the North no longer permitted ecclesiastical union. Winans also promoted the Methodist mission to the Choctaw Indians and considered Andrew Jackson's removal policy "crooked and disgraceful" (p. 104). Winans found himself in complete disagreement with Jacksonian policies and became an outspoken Whig enthusiast, even letting himself be placed in nomination as a Whig candidate for Congress in 1849. Winans lost the election, as he had earlier failed in being elected to his church's bishopric. He was often disappointed

in his brother Methodists, fellow Mississippians, and American countrymen, and he expressed his sentiments in voluminous correspondence.

Ray Holder, a retired Episcopal clergyman, stands back and lets Winans' own words tell his story. With almost no analysis, Holder has utilized the extensive Winans Collection of the Mississippi Conference Historical Society at Millsaps College, Jackson, to present the career of William Winans. This collection consists of Winans' unfinished autobiography, his journals, letters, and published works. Holder has garnered information from the whole and arranged hundreds of relevant quotes in chronological order. Essentially he has served as silent editor, completing Winans' autobiography; there are neither footnotes nor exact citations to the material. Holder has no pretensions about what he has done. He has quite effectively alerted historians to the importance of Winans' career and demonstrated the enormous richness of his papers. The present volume was intended to motivate "a new probe into the making of the southern 'Mind'" on the part of "competent students" (p. 220), and "to serve as a possible introduction to a volume of edited selections from [Winans'] manuscripts." If such studies follow, then Holder's purpose, and the purpose of scholarship, will have been well served.

JOHN B. BOLES
Rice University

DONNA HILL. *Joseph Smith: The First Mormon*. Garden City, N.Y.: Doubleday and Company. 1977. Pp. xviii, 327. \$12.50.

Given the current interest in cultural history and the history of religion, a scholarly biography of Joseph Smith is long overdue. Previous biographies produced by Mormons have either ignored controversial issues (John Henry Evans, *Joseph Smith: An American Prophet* [1933]) or were written to satisfy the faithful (Francis Gibbons, *Joseph Smith: Martyr, Prophet of God* [1977]). In addition, anti-Mormon literature has enjoyed a long and prosperous career. Stretching from early works like Alexander Campbell's *Delusion: An Analysis of the Book of Mormon* (1832), this literature has included more recently Wesley Walters' *New Light on Mormon Origins from the Palmyra (N.Y.) Revivals* (1967). Somewhere between is Fawn M. Brodie's *No Man Knows My History: The Life of Joseph Smith* (rev. ed., 1971). The problems of Brodie's work have been dealt with elsewhere and need not concern us here. (Marvin S. Hill, "Secular or Sectarian History? A Critique of No Man Knows My History," *Church History*, 43 [1974]: 78-96).

Donna Hill's biography of Joseph Smith is a

great improvement on previous work. Unlike books designed for Mormon consumption it deals with both the blemishes and the beauties of the Mormon experience. It is not concerned with proving or disproving Joseph Smith's claims, and perhaps most important, unlike Fawn Brodie, Donna Hill has recognized that Joseph Smith was a religious leader whose experiences and teachings affected the spiritual lives of millions.

Perhaps the strongest feature of Hill's biography is its recognition of the unity of the various aspects of culture evident not only in the experiences of Joseph Smith but also in the expectations of those who followed him. Thus, Joseph Smith could quite easily combine money digging, peepstones, and mystical experience into a whole which was congruent to him and those who shared his worldview. Hill recognizes that the dichotomy between secular and sacred which is central to Brodie's interpretation would have been unrecognizable to Joseph Smith and his followers.

Indeed, it was the antipluralistic implications of Mormonism which caused most conflict between Latter-day Saints and outsiders. Mormon millennialism infused with the Kingdom of God concept, unity in political matters, and communitarian economic ideals led inevitably to conflict. From Joseph Smith's point of view, these were all religious matters and the attacks on the Latter-day Saints constituted religious persecution. In addition, social arrangements like plural marriage within the Mormon religious community contributed to both external and internal stress.

Though it is this point of view which provides the strength of the biography, extensive research underpins the narrative with sound evidence. Showing a familiarity with secondary works, the author also did extensive research in manuscript sources of the archives of the LDS and RLDS Churches and at such universities as Yale, Stanford, Brigham Young, Utah, and California.

The work is not without its shortcomings. One problem derives from the author's occasional tendency to use a contemporary source without indicating how the person cited would have known the information. Another comes from the absence of a broad interpretive framework. The book is, nevertheless, good narrative history, and Donna Hill's work should remain as the standard biography of the Mormon prophet for years to come.

THOMAS G. ALEXANDER
Brigham Young University

CULVER H. SMITH. *The Press, Politics, and Patronage: The American Government's Use of Newspapers, 1789-1875*. Athens: University of Georgia Press. 1977. Pp. xv, 351. \$18.50.

Journalism historian Frank Luther Mott called it "the dark ages of American journalism," reflecting modern journalists' distaste for close affiliation with politicians. To most early American editors and politicians, at least up to the Civil War, the patronage and political press, however, was a necessary and desirable national institution. "The engine is the press," wrote Jefferson in 1799, and with liberal postal policies favoring newspaper exchanges and distribution, the press facilitated the nation's vital political intercourse for nearly a century from a position near the center of government.

Culver H. Smith, occupied with various aspects of newspaper history during much of his professional career, has succeeded admirably in tracing "the American government's use of newspapers" from 1789 to 1875. The resulting work, however, violates this subtitle's implication of a one-way relationship by describing a dynamic interdependence between these two institutions.

Smith's narrative, thus, casts no suspicion on the intertwining of press and government activities, thereby remaining true to the spirit and perspective of the age under study. A far different, and gravely inaccurate, account of these activities would arise from a 1970s viewpoint based on editors' and politicians' frantic search for distance between each other—their perpetuation of a mythical adversary relationship.

Two avenues of interaction between press and government have long been studied—newspapers as presidential and party mouthpieces and newsmen as reporters of Congressional debates—but Smith provides a valuable overview of these practices, lasting from 1789 and the appearance of the Federalists' *Gazette of the United States* in New York City to the 1860 establishment of the Government Printing Office and Lincoln's refusal to anoint a personal news organ. Although general and fast-paced, Smith's treatment permits the introduction of presidents' editors and Congresses' reporters in brief but intimate glimpses of personality, background, and relationship to governmental power.

This book's greatest contribution, however, is its detailed revelations of a third, lesser-known press and government relationship during this period—State Department use of selected newspapers in each state and territory to publish the laws of Congress. The practice, introduced in 1789 to inform the citizens of every state through their local newspapers of the measures passed by their Congress, came to an end in 1876, adjudged by Congress as being too expensive, overly tinged with patronage and partisanship, and threatening to an independent press.

Smith treats his reader to a close examination of this State Department practice as it unfolds in the cabinet trays and handwritten volumes of the Na-

tional Archives' General Records of the Department of State, a manuscript collection previously unopened for research on this topic. The bulk of the appendix matter is devoted to periodic lists of participant newspapers, a useful illustration of the text description.

Thoroughly researched and documented and faithful to the assumptions ingrained in the subject matter, Smith's work is a valuable and comprehensive contribution to a literature that, until now, has focused only on specific periods, practices, administrations, or personalities. The book should also be required reading as background for the historian preparing to take his initial plunge into the murky files of eighteenth- and early nineteenth-century American newspapers.

RICHARD A. SCHWARZLOSE
Northwestern University

WESLEY NORTON. *Religious Newspapers in the Old Northwest to 1861: A History, Bibliography, and Record of Opinion*. Athens: Ohio University Press. 1977. Pp. xi, 196. \$12.50.

For the most part, newspaper historians have ignored the more specialized press—the foreign-language, literary, and religious journals. It is certainly true that they were more ephemeral than the general press, and not as widespread, and yet they were a significant part of our newspaper history. Wesley Norton has chosen the religious newspapers of the old Northwest for study and has lifted the curtain on a significant aspect of journalism history. Not only has he given us an institutional study—a cultural portrait—of the religious press, he has also provided us with a bibliography of such newspapers in that section of the country which is useful to the researcher if not to the general reader.

Withal, the job of religious editors was much like that of general editors; much of their news was secular, and it included oddments and a poetry corner. Like the general editor, who was highly politicized, the religious conductor faced competition and controversy. Journals proliferated within denominations, much as with political factions, and religious editors engaged in controversies with Catholics, Universalists, Mormons, and Millerites, just as did the Whig, Democratic, and Republican editors. Consequently, their newspapers were unstable and made no money. The religious editor even advertised, although he was a little more choosy than the political printer. He thought of himself as overworked, unappreciated, and beleaguered. He did not seem to like his job; neither did his political counterpart.

Where he differed was perhaps in his prohibi-

tory mentality. He opposed dancing, acting, drinking, smoking, July 4th, even Christmas—in short, anything that was any fun. He abhorred women's rights, the cure for which was matrimony and the maternal instinct. The best day for him was the somber Sabbath.

His major purpose was moral uplift. In this, like his political colleague, he was independent in all things and neutral in nothing. The religious editor wanted to be nonpartisan, but he probably was political; he wanted to be denominational, and probably was ecumenical. He achieved a denominational consensus, but often crossed religious boundaries. One great movement which he joined was the antislavery crusade. Norton believes that these religious editors made moral outrage a national institution. Their legacy was the conviction that society can be improved, even when it is in travail. Out of sin comes salvation.

This, says Norton, was the editor's contribution to our own time. Today, though we may accept the idea of the perfectibility of society, it is hard to pin the old-time religious editor down. We certainly have rejected his attitude toward amusements, and we do not keep the Sabbath. Was he essential to the abolitionist crusade, or the later civil rights movement? It is very difficult to say.

WILLIAM H. LYON
Northern Arizona University

ANN DOUGLAS. *The Feminization of American Culture*. New York: Alfred A. Knopf. 1977. Pp. x, 403. \$15.00.

By "feminization" Ann Douglas means the complex process which has made American culture sentimental and anti-intellectual, conditions which have resulted in a nation of culture consumers rather than producers. This brilliant social history of readers' tastes as well as of writers, primarily from 1820–75, demonstrates that this period witnessed the eclipse of rigorously intellectual Calvinist theology and the emergence of a pious religiosity expressed in devotional verse and sentimental, domestic novels. Liberal ministers in the Northeast, middle-class women, and some of the popular male writers of the period joined together unintentionally to initiate the self-conscious mass culture of today so dependent for its commercial success upon the mass media and advertising techniques.

Appealing to the popular taste in religion was forced upon the theologians and the ministers, according to *The Feminization of American Culture*, by their disestablishment by 1820. By 1850 women had become just as surely disestablished through the loss of some of their legal rights, exclusion from

a number of occupations, and a decreasingly productive role in society. Douglas' book is feminist in finding an indirect cause for the damage done by these women in the previous wrongs forced upon them by the male-dominated society at large. The damage done by their sentimental, pious novels was to make much more difficult of achievement "a fully humanistic, historically minded romanticism" as "the logical antagonist [and successor] of Calvinism" (p. 13).

The liberalization of religion in the latter eighteenth and nineteenth centuries took away much of the force and virility of Calvinism, while at the same time making ministers increasingly insecure and forcing upon them the need to win approval from their congregations. The liberal ministers and the middle-class women formed an unconscious alliance both imitating and competing with each other in their roles, trying to compensate for their losses through appeals to the emerging mass market of lady readers. Both ministers and women wrote historical works and biographies that were in reality a "flight from history" (p. 188). Their novels displayed a flight from a balanced view of life to the exaggerated emotions wrung from disappointed love, suffering wives, wayward husbands, and the deaths of children.

Douglas presents a detailed analysis of the lives and works of representative ministers and women writers, including Horace Bushnell, Orville Dewey, Edward and Henry Ward Beecher, Harriet Beecher Stowe, Lydia Maria Child, Lydia Sigourney, "Fanny Fern," and "Grace Greenwood." Thirty ministers and thirty women comprise the "control groups." The analysis is based on such wide reading in the popular books and magazines of the period that the evidence becomes clear and convincing by sheer quantity as well as by the logic and insights of the author.

Margaret Fuller was one woman who disavowed the rising tide of sentimental fiction and fantasy through her dedication to action and to the real world. Even her famous "Conversations" led to a discovery of truths, not fantasy, and she plunged wholeheartedly into the Italian revolt against Austria and France in 1849. All her intellectual pursuits fostered criticism, not consumerism.

Herman Melville's novels are a "revolt against the reader" of his day because the early South Sea adventures and *Moby-Dick* celebrate masculine adventures and attitudes; in *Pierre*, "Melville turned decisively and openly against the middle-class sentimental-minded feminized reading public he had essentially tried to evade or educate in his previous work" (p. 309). Several magazine stories conform on the surface to the popular taste while bitterly satirizing it through Melville's irony. *The Confidence-Man* showed the triumph of sentimentality

in America and reduced Melville to illness and silence for years before he regained sufficient composure to write some poems inspired by the Civil War and then *Billy Budd* at the very end of his life.

The Feminization of America is an important book showing convincingly how American culture since 1850 has become increasingly impoverished as sentimentality and anti-intellectualism have almost choked out ideas, genuine art, and informed taste among the populace.

RUSSELL E. DURNING
Northern Illinois University

JOSEPH F. KETT. *Rites of Passage: Adolescence in America, 1790 to the Present*. New York: Basic Books. 1977. Pp. ix, 327. \$16.50.

Joseph F. Kett analyzes several critical transitions experienced by young people since the early nineteenth century. The years 1790-1840 witnessed the uprooting of youth from the countryside, expanding occupational choices, and increasing disorder and violence in the social and educational institutions of the young. At the end of the century economic opportunities for middle- and lower-class youth diverged. In order to ensure that their children would find a place in the economic world, middle-class parents responded with new strategies, emphasizing adult-controlled youth organizations and institutions, and stressing passivity and acquiescence as desirable traits among the young. An extensive reclassification of young people as adolescents took place between 1890 and 1920, but the historian who would explain this phenomenon, Kett laments, must look to "the compost heap of American cultural life" rather than to "the mature fruits of creativity" (p. 6).

Defining adolescence as "the period after puberty during which a young person is institutionally segregated from casual contacts with a broad range of adults" (p. 36), Kett finds that adolescence hardly existed in the early Republic. Instead, the young experienced overlapping periods of semidependence (from about age 10 to 21) and indecision (from 15 to 25). Semidependence was no smooth, upward path to adulthood, for there were severe discontinuities in young people's lives. However precociously they might have mingled with their elders in many civic, religious, and social contexts, they matured less rapidly in others: physical growth was slower; pressures to choose vocations were less intense; and traditions of deference and subordination were stronger. The young encountered both freedom and subjection, assertion and submission, in their sporadic home leavings and returns, the episodic nature of their schooling, their tendency to change academies,

apprenticeships, and employment, and the sometimes protracted waxing and waning of religious anxiety before conversion. If standardized modern adolescence makes difficult the formation of individual identity, the nineteenth-century period called "youth" may have been difficult for precisely opposite reasons: the anomalous and diffuse nature of their roles may have hastened the young toward adulthood, if only to escape the uncertainties of semidependence.

While Kett sympathizes with the problems facing early nineteenth-century youth and admires some of their responses, he is hardly sympathetic to adults. Beginning with antebellum youth counselors who stifled precocity while urging "decision of character," Kett analyzes the fears of middle-class adults who sought to prolong immaturity by emphasizing adult leadership, submissiveness on the part of the young, and segregation from adult affairs. G. Stanley Hall's seminal yet highly idiosyncratic *Adolescence* (1904) was but one manifestation of the process by which anxiety-ridden adults invented adolescence. Young people were the victims, not beneficiaries, of this invention, for early twentieth-century custodial institutions outlived the ideas and impulses of their founders and continued to promote norms of conformity, anti-intellectualism, and passivity. Despite efforts by the inventors of adolescence to universalize the experience, Kett observes that "many young people voted with their feet and resolutely pursued lifestyles that deviated from the ideal of conformist and ingenuous adolescence" (p. 246).

Kett's principal focus on white, middle-class, male youth, however regrettable in other respects, makes pragmatic sense in light of one of his central objectives: to examine both adult thought about and the actual experience of young people. The sources are certainly richest and most accessible for middle-class males, and Kett realizes his objective effectively and imaginatively for the early nineteenth century. However, while his examination of adult attitudes in the late nineteenth and twentieth centuries is detailed and persuasive, he provides relatively little information about the experiences of the young people upon whom adolescence was imposed. Thus, his criticism of the inventors of adolescence for failing to make an empirical assessment of the actual behavior of young people can in part be applied to his own work. In addition, the very narrowness of his definition of adolescence as a twentieth-century cultural artifact may obscure essential continuities in the experience of young people. In this context, one should note that the rationale for beginning the book at 1790 is not fully or clearly articulated. Despite these limitations, *Rites of Passage* is an important contribution to social and intellectual his-

tory. Its scope, richness of data, and sometimes playful humor commend it to a wide audience.

ROSS W. BEALES, JR.
College of the Holy Cross

DONALD S. SPENCER. *Louis Kossuth and Young America: A Study of Sectionalism and Foreign Policy, 1848-1852*. Columbia: University of Missouri Press. 1977. Pp. vii, 203. \$12.50.

Upon his arrival in New York City in December 1851, Louis Kossuth was greeted by wildly enthusiastic torchlight parades and by placards demanding that the United States intervene in behalf of the Hungarian revolution. By the time he quietly left the country under an alias in July 1852, most Americans had lost interest both in Kossuth and the cause he represented. After tracing the origins of the Young America movement, Donald Spencer describes Kossuth's seven-month tour and assesses the ideological and sectional patterns of the American reaction to the Hungarian's abortive effort to procure diplomatic, economic, and military assistance to rekindle his dead revolt. Initially Kossuth received a warm response in the North and West but a frigid one in the South. Later he found his crusade increasingly opposed by a strange assortment of radical abolitionists, conservative Southerners, and national Whig leaders. Even the Young America Democrats, who provided the bulk of Kossuth's adherents, deserted the cause in the name of party unity as the presidential election approached in 1852. Kossuth and Young America, Spencer thoughtfully concludes, were not able to alter the course of United States foreign policy primarily because the Hungarian's specific demands compelled Americans to contemplate moving beyond evangelical rhetoric to actually violating the admonitions against intervening in European affairs set forth in President George Washington's Farewell Address. When confronted with reality, the "Magyar mania" collapsed.

Spencer's well-written and attractively illustrated monograph contributes to our understanding of American society and foreign policy at mid-century, but it contains defects which limit that contribution. The author's research in such printed primary sources as congressional documents, newspapers, and reminiscences is outstanding, and he has investigated such relevant private papers as those of Kossuth. There are, however, significant gaps in the research. For example, though Spencer skillfully analyzes the proceedings of the annual banquet of the Jackson Democratic Association held on January 8, 1852, he does not use the even more important proceedings of the congressional banquet for Kossuth,

which met on the previous day. Moreover, neither the private papers of William H. Seward nor those of Daniel Webster have been consulted. In terms of analysis, Spencer probably has overestimated Webster's commitment to an evangelical foreign policy and underestimated his desire to use foreign policy to promote domestic unity while adhering to the principle of nonintervention. Spencer also mistakenly has Lewis Cass being recalled as minister to France because of his protest against the Quintuple Treaty, when in fact Cass' position had been sanctioned by the Tyler administration before he resigned. Finally, the ambiguous Cass and Seward resolutions of 1852 are misinterpreted as calling for a military alliance. These related shortcomings of research and analysis detract from what is otherwise a lively and valuable study.

KENNETH E. SHEWMAKER
Dartmouth College

MICHAEL MEYER. *Several More Lives to Live: Thoreau's Political Reputation in America*. (Contributions in American Studies, number 29.) Westport, Conn.: Greenwood Press. 1977. Pp. xii, 216. \$14.95.

Michael Meyer has examined, carefully and analytically, the uses to which writers from the 1920s to the present have put Thoreau. The uses are in diverse ways "political," but Meyer has not confined himself to writers who have specific ideologies to promote, or to those parts of Thoreau's work that make direct social or political statements: a critical interest in Thoreau the naturalist or Thoreau the literary artist, Meyer presumes, may reveal a good deal in the way of the critic's larger tastes and priorities. He looks through the twentieth-century discoveries of Thoreau as Walden observer, as advocate of civil disobedience, as pacifist of sorts, and as defender briefly of abolitionist violence, and follows the fortunes of this enigmatic or contradictory mind as libertarians, liberals, radicals, and literary formalists have variously reacted to it. The result is a study valuable not only in its explicit topic but in making us recognize the responsibilities and risks that come when writers, such as Meyer's commentators on Thoreau, adopt the basically legitimate strategy of remarking on one system of thought for the purposes of furthering another.

Meyer has chosen to stand fairly far apart from his materials, treating the commentaries on Thoreau essentially as objects of scholarly inquiry. He announces quite early that to present a systematic idea of Thoreau would require a separate book. It would not be out of place, however, respectfully to engage each critic and his or her ideas, however stumbling, and not only in regard

to the question of whether the critic has gotten Thoreau right but, whenever proper, on the political or literary issues at hand. I feel warmest toward Meyer when he lightly intrudes himself into the discussion. Concerning an attempt to appropriate for the civil rights movement Thoreau's later flirtation with the possibilities of violence, Meyer reminds the reader of the distinction between a willingness to die for a cause and a willingness to kill for it; at the end of the study he suggests that Thoreau's "apolitical temperament" brought "the impulse to champion violence as a means of surgically removing evil from the world" (p. 192). Such touches indicate that Meyer has his own complex notion of the moral uses we could make of Thoreau.

THOMAS R. WEST
Catholic University of America

CONSTANCE NOYES ROBERTSON. *Oneida Community Profiles*. Syracuse, N.Y.: Syracuse University Press. 1977. Pp. ix, 146. \$10.00.

This book, Constance Noyes Robertson's third on Oneida, presents difficulties for the historian: some of the most interesting material is based upon documents in Robertson's possession and not available to other historians. Moreover, there are no footnotes. Robertson, granddaughter of Oneida's founder and leader, John Humphrey Noyes, admits that "much of this book is strictly personal" (p. viii), and not only for the reasons she cites in the preface. Her interpretations of Noyes, the founding of Oneida, life in Oneida, the personal histories of George and Mary Cragin (two of Oneida's central figures), and the death of Mary Cragin lack critical analysis and add virtually nothing to our knowledge of those people and events. When Robertson relies upon sources familiar to others, it is evident that she has approached her materials with restraint, thus often denying these characters and events the richness and complexity they possess.

Robertson's best chapters deal not with Oneida, but with Marcus Worden's attempts to cope in two of Oneida's branch communes: Manlius and the second Putney venture. Social, sexual, and economic difficulties plagued Worden, suggesting that it was difficult to establish and maintain the ideal of communal harmony. Robertson's lengthy quotes from "M. L. Worden's Log" (pp. 101-05) are especially rewarding.

Robertson has also provided some new, yet brief, material on Noyes in his Brooklyn commune in 1849. Noyes, in "The Brooklyn School," is depicted as an active leader, having closer contact with Oneida than we have previously supposed.

Oneida's members visited Noyes from week to week, bringing their emotional problems to him, seeking his critical and healing powers. At one point his followers' emotional demands became too much for Noyes, and, as was his wont, he retreated. He warned them that they would have to curb their demands upon him because "he needed a warm, genial atmosphere, instead of which he was surrounded by fear" (p. 73).

Oneida Community Profiles does not add appreciably to our knowledge of Oneida or Noyes' own complex motivations, but it does contribute to our understanding of life in the branch communes.

ROBERT DAVID THOMAS
University School
Chagrin Falls, Ohio

GARRY E. CLARKE. *Essays on American Music*. (Contributions in American History, number 62.) Westport, Conn.: Greenwood Press. 1977. Pp. xviii, 259. \$16.95.

In this volume of essays, Garry E. Clarke attempts, as many have done before him, to define American music and to find some elements common to an American style. But although he argues that there are national characteristics in a great deal of European music and that there are easily recognizable folk and rhythmic elements in some American music, in the end he can do no more than accept Virgil Thomson's dictum that "the way to write American music is simple. All you have to do is to be an American, and then write any kind of music you wish."

Readers who expect something new or striking will be disappointed. The essays are earnest in tone, well written, and copiously annotated, but they contain few new insights or original judgments. Most of the material has been amply studied and discussed in such indispensable works as Wilfrid Mellers' *Music in a New Found Land* (1965), Gilbert Chase's *America's Music* (2d ed., 1966), H. Wiley Hitchcock's *Music in the United States* (1974), and even John Tasker Howard's pioneering *Our American Music* (1929).

Clarke disclaims any intention of writing a history of American music, although his eight essays range from the eighteenth-century New England composers to the avant-garde of the 1970s and are arranged chronologically, so that in a sense one follows the evolution of American music through the author's selection of representative and significant composers and movements. Clarke has obviously examined subjects that particularly interest him, and the choices are very interesting: The Yankee Tunesmiths; Louis Moreau Gottschalk; The American-Europeans; Charles Tomlinson

Griffes; Charles Edward Ives; Quincy Porter: Composer-Professor; Virgil Thomson; and The New Eclecticism. These essays are framed by a prologue and an epilogue which are attempts at general statements, but which are rather inconclusive.

The most interesting pieces are the last three. The essay on Quincy Porter is a generous tribute to a composer who was widely admired by musicians, but whose music is little played or known today. Porter is studied both as a composer and as an influential teacher in a major university. The essay on Thomson is curious; Clarke does not seem to be able to make up his mind about his subject and hardly does justice to Thomson's wit and originality. The final essay discusses the work of John Cage, Milton Babbitt, Earle Brown, George Crumb, and George Rochberg, as well as some composers of a younger generation, such as James Drew and Robert Morris. Clarke is enthusiastic about the new esthetic and the search for a new synthesis and is optimistic about the future of a "great" American music.

The book includes extensive notes, quotations and musical examples, and shows evidence of careful study. One wonders in the end, however, to what audience the book is addressed. It will not be of great use to students nor is it likely to attract the attention of serious scholars.

RICHARD FRANKO GOLDMAN
Peabody Institute of the City
of Baltimore

ROY W. MEYER. *The Village Indians of the Upper Missouri: The Mandans, Hidatsas, and Arikaras*. Lincoln: University of Nebraska Press. 1977. Pp. xiv, 354. \$14.95.

The Indians of the Fort Berthold Reservation in North Dakota, officially known now as the Three Affiliated Tribes, receive full scholarly treatment in this excellent book. Village Indians, who lived in the bottom lands along the upper Missouri River for a millenium or more, the Mandans, Hidatsas, and Arikaras maintained a semiagricultural existence in the area we normally associate with the nomadic horse tribes like the Sioux. Because of their prominent location along the river, these tribes were observed and described by white traders and other visitors (beginning with the Frenchman La Vérendrye in 1737), and the body of material on them is extensive. Meyer has skillfully relied on these reports, as well as on the works of archeologists and anthropologists and the vast resources of the National Archives, to present a well-rounded account of the aboriginal life of these related but originally distinct tribes and the

changes that occurred after they came in contact with whites.

As is inevitable in a history which has to rely on documents produced by whites, the Indians here are treated more as passive objects than as controllers of their own destinies. Meyer is aware of the problem, however, and he writes: "However much one might wish to tell the story from the 'inside,' the very nature of the sources necessitates telling it largely from the 'outside'. . . . Since the most traumatic event in the history of the American Indian was the occupation of the western continents by Europeans, this book treats the history of the Three Tribes mainly in terms of the effects of that occupation" (p. xii).

Three effects of the white invasion were of special importance and receive extended treatment in this book: the smallpox epidemic of 1837, which nearly wiped out the Indians and caused the consolidation of the three tribes; the allotment of the communal lands under the Dawes Act of 1887, which dispersed the Indians from their villages to individual farms scattered over the landscape; and finally, the construction of the Garrison Dam on the Missouri in the 1950s, which forced the Indians out of their lands along the river into the higher prairies, where agricultural existence was much more precarious.

Meyer writes with deep sympathy for the Indians, and he severely condemns, implicitly and often explicitly, the government actions that disrupted the lives of the Indians. But he is balanced in his judgments, noting as well the continual warfare of the tribes among themselves and with the Sioux and the intense factionalism within the tribes that contributed to tribal problems.

One of Meyer's strong points, in his earlier history of the Santee Sioux as in this book, is his willingness to carry his account right up to the present. Unlike so many histories of Indian tribes which end somewhere near the end of the nineteenth century, this book courageously moves into the twentieth century, with accounts of economic, political, and educational developments, which are less dramatic than nineteenth-century buffalo hunts and intertribal warfare, but which make us understand the Indians as they are today rather than as stereotypes from the past. Such a broad sweep enables Meyer to end what is essentially a sad story on a positive note: "With employment rising, Indian use of reservation resources increasing, the number of young people acquiring post-secondary education growing year by year, and pride in their Indian heritage asserting itself in various ways, the Three Tribes would seem to have ample cause for looking to the future with optimism" (p. 261).

The Village Indians of the Upper Missouri is one of

the best of a growing list of competently done tribal histories.

FRANCIS PAUL PRUCHA
Marquette University

DANIEL F. LITTLEFIELD, JR. *Africans and Seminoles: From Removal to Emancipation*. (Contributions in Afro-American and African Studies, number 32.) Westport, Conn.: Greenwood Press. 1977. Pp. x, 278. \$15.95.

This book contributes to the study of ethnic relations between American Indians and Afro-Americans by focusing on the Seminoles between 1835 and 1865. David F. Littlefield shows that blacks were a major factor in Seminole history after runaway slaves began to seek refuge with the Florida Indians. Although most of these blacks were held as slaves by Seminoles, enslavement was so benign that it was hardly slavery at all: blacks generally lived in separate towns where they owned their own farms and livestock, were not sold or punished, and sometimes intermarried with Indians. They were treated well because they provided important services to the Seminoles, as warriors, interpreters, spies, and food producers.

American military leaders in the Second Seminole War gradually realized that the Seminoles could not be removed unless their black allies were allowed to go with them. Pressures from white slaveholders for the return of their runaways were ignored, and about five hundred blacks were removed with the Seminoles to Indian Territory. Many of the blacks had clear commitments from the Army that if they surrendered they would be free.

Once they arrived in Indian Territory the Indians no longer needed the blacks as allies, and the alliance began to weaken. Moreover, blacks who were convinced they were free did not wish even the benign enslavement by Seminoles. Nevertheless, their relations would probably have continued to be close if it had not been for neighboring whites, Creek Indians, and Cherokee Indians, who felt threatened by the high status accorded to Seminole blacks. The Creeks in particular tried to restrict black rights and raided Seminole black settlements for white slave-traders. At this point proslavery officials in the U.S. government essentially negated earlier agreements with the blacks, and decided in 1848 that Seminole blacks did not have the right to enter into binding agreements because they were property. Consequently, many independent-minded blacks escaped to settle in Mexico.

Those blacks who remained in Indian Territory in the 1850s were subject to periodic slave raids,

and were not secure until after the Civil War. Most of them, and the majority of Seminoles, sought refuge within Union lines during the war. In 1865 they were declared free and equal members of the Seminole Nation.

The author uses a traditional historical narrative approach. He employs government documents effectively, but there is no quantitative analysis of the available statistics. Certain topics, for example the 1865 treaty, are presented in a confusing manner, and the mass of names is unnecessarily detailed. The relative lack of ethnographic analysis of day-to-day lifestyles and kinship relations also weakens the book. Despite these faults, this monograph makes a substantial contribution to a neglected field.

WALTER L. WILLIAMS
University of Cincinnati

SAMUEL CARTER III. *Cherokee Sunset, A Nation Betrayed: A Narrative of Travail and Triumph, Persecution and Exile*. Garden City, New York: Doubleday and Company. 1976. Pp. xi, 318. \$9.95.

It might be argued with some validity that what the field of Indian history needs least at this point is another account of the Cherokee removal. Indeed, upon first perusal it becomes evident that *Cherokee Sunset* is not the result of extensive primary research; nor does it present any new ideas or theories. The book is simply a popular account of the Cherokee experience during the period of removal, written for a general audience, and confined largely to the Indian viewpoint. Despite this, Samuel Carter, a writer by profession, has put together a worthwhile book.

Cherokee Sunset is a concise narrative of the fight of these peaceful people to remain in their southern homelands. Carter examines their extensive efforts to conform to white civilization—the brilliant development of a written language, a workable form of government, and an agrarian economy. Most emphasis is placed on telling the story of the Cherokee attempt to preserve their society through the activities of their leaders—John Ross, Elias Boudinot, Major Ridge—accompanied by the aid of white missionaries like Samuel A. Worcester. The narrative goes through the familiar fight with Andrew Jackson and the state of Georgia, the Supreme Court fiasco, and finally the actual removal and resettlement in Oklahoma and Arkansas. Anyone acquainted with the literature of the Cherokee will recognize both the story and the sources.

The value in this book comes in the succinct and well-written account. Although concentrating on the Cherokees, and thus omitting much of the national controversy over removal, the story gives

a real insight into the tragedy of forced relocation without being overly emotional. One of the better portions concentrates on the internal split between those like Major Ridge and Elias Boudinot, who decided it was best for the Cherokee to yield to the inevitable and go west under favorable conditions, and the majority of the Cherokees, who following John Ross, decided to resist to the bitter end. Carter sees no heroes or villains in this unfortunate controversy, and he treats those who signed the fraudulent treaty of New Echota (1835) with understanding and compassion. Although the treaty faction has been much maligned, they were in fact some of the most respected leaders of the nation and took an action they considered in the tribal interest, despite the fact that it would mean their assassination—a fact that is often overlooked. Because of the judicious coverage of the internal stress on the Indians, as well as a good glimpse at the way they were mistreated by the government and the Georgians, *Cherokee Sunset* is likely to replace Dale Van Every's *Disinherited* as the best popular account of removal.

The book is not, however, without its faults. There are a number of factual errors, including a statement that the Hopewell Treaty in 1785 established their independence as a "sovereign" nation (p. 1). In addition, the advancement of Cherokee civilization was not as all-encompassing as one is led to believe, some might question the way the statistics on the number of deaths that occurred during removal are presented, and little mention is made of white support for the Cherokee outside the South. Still, these problems fail to hamper the overall quality of this general account.

ROBERT A. TRENNERT, JR.
Arizona State University

RANDOLPH B. CAMPBELL and RICHARD G. LOWE. *Wealth and Power in Antebellum Texas*. College Station: Texas A & M University Press. 1977. Pp. xv, 183. \$14.50.

This study combines the smooth writing of historians with numerical data from two random samples of five thousand free heads of families in 1850 and 1860. Procedures are described clearly, in comfortable, nontechnical language. The lucid text helps the non-statistically-minded reader through the plethora of tables and reassures him as to the unbiased character and adequacy of size of the sample. The authors give supporting data for the thesis, presented in the September 1976 *Journal of American History*, that southern wealth was not more unequal than in such a northern state as Wisconsin and many tables rich in specific content as to who held the wealth of various kinds,

and relationships between political leadership, wealth, and occupation. The samples are drawn from the manuscript records of the U.S. 1850 and 1860 censuses for Texas, generally following the paths opened by such economists as William Parker, Robert Gallman, and Lee Soltow. Gini indexes of wealth inequality are computed for the entire sample, for population subgroups, and for total wealth and several subcategories. Absolute wealth classes, rather than quintiles or deciles, are shown. Neither regression equations of interrelationships between variables nor standard deviations of sample means have been calculated, but t-tests were made of selected characteristics of the sample and of the complete census.

The universe sampled is the 93 percent of free heads of families found in the eastern two-fifths of Texas, which contained 99 percent of the slaves in both census years. The samples were stratified by four regions, within that area, of differing climate, soil, age of settlement, and population density: upland, coastal, north-central prairie, central and southern prairie and plains. The tables are straightforward percentage distributions or mean values for items selected for analysis. Variables tabulated include: from the census, place of birth, age, sex, race, occupation, amount of real and personal property, number of slaves owned, farm ownership and acreage, cash value of farm, value of farm implements and livestock, and size of various crops; from political records, officeholding, attendance at party or constitutional conventions. Such measures of political leadership follow in part work by Jackson Turner Main and other social historians.

The study demonstrates convincingly that there was substantial inequality in wealth among free families in the relatively new state of Texas, but it was of about the same order of magnitude as found by Soltow in Wisconsin. It shows that inequality in the large towns exceeded rural inequality and reflected dominance of slaveowners and merchants, chiefly native born. Although the percentage of wealth held by the top 1 or 2 or 5 percent was somewhat lower than in older cities, the Gini index for concentration of wealth over the entire range was only slightly lower for large Texas towns than for northern, eastern, border cities and New Orleans.

ALICE HANSON JONES
Washington University

MARGARET SWETT HENSON. *Samuel May Williams: Early Texas Entrepreneur*. College Station, Texas: Texas A & M University Press. 1976. Pp. xvi, 190. \$10.00.

The name of Samuel May Williams is not enshrined in the Texas Hall of State nor immortalized in the name of a county or town; Williams may well be the most undersung and underappreciated of all historic Texans. Margaret Henson has attempted to rescue him from his undeserved oblivion. Her effort is valiant and successful, though, reader tastes being what they are, Williams is unlikely to become a household name in Texas, much less throughout the nation.

Williams was a civilized father of Texas, which does not fit the stereotype of a Texas frontiersman. A native Rhode Islander, he had worked at length in Baltimore and Buenos Aires, which should constitute some variety of experience, spoke Spanish fluently before he ever stepped ashore in Texas, had dealt with the pirate Jean Laffite, and had served a mercantile house in New Orleans. The only Texan qualities about him were that he left the United States owing a large personal debt, and he arrived in Texas under an assumed name.

But Williams' name was soon known to Anglo-Texans, for as secretary to the *empresario* Stephen F. Austin, he performed the paper work for Austin's colony and signed most of the deeds to land for the new settlers. In time he allied with another new Texan of entrepreneurial tendencies, Thomas F. McKinney, to form a mercantile house. When Texas revolted against Mexico, the upstart Texas government had no army, no munitions, no money, and especially no credit. The revolutionary effort might have foundered, except that the mercantile firm of McKinney and Williams offered to stand between the newfangled Republic of Texas and its needs. Merchants along the coast of the Gulf of Mexico and the Atlantic Ocean, undisposed to advance anything to any entity as nebulous as the Texas government, would advance materiel and credit to McKinney and Williams, who guaranteed to pay if Texas could not.

In the victorious revolution that followed, McKinney and Williams picked up approximately one-tenth of the cost of the war. As of this writing neither the merchants nor their heirs have ever been paid. The odds are that they never will be. Yet a bloc of land the size of France might have remained a part of Mexico and the subsequent war against Mexico might never have given the United States such dubious blessings as Los Angeles if McKinney and Williams had not come forward.

Williams also holds a place in frontier banking history. Since Texas was born at the tag end of the Jacksonian era, Anglo-Texans distrusted banks. The Republic of Texas constitution forbade banking, as did successive state constitutions from 1845 until during Reconstruction. As partial compensation the several Texas governments did, however, permit Williams to bank, and at times

his was the only bank in the burgeoning Southwest.

The author has placed Williams in context, thereby enlarging our understanding of the period from Anglo-American emigration in the 1820s to the eve of the Civil War. The book is overdue, but the result is worth the wait.

JOE B. FRANTZ
University of Texas,
Austin

MICHAEL P. JOHNSON. *Toward a Patriarchal Republic: The Secession of Georgia*. Baton Rouge: Louisiana State University Press. 1977. Pp. xxiv, 244. \$15.00.

In recent years, many historians have reconsidered the Southern decision for secession, using that climactic event as a probe to explore the inner dynamics of slave society. Michael P. Johnson uses this approach to develop a markedly different interpretation of disunion. Secession was not a product of race phobias, he writes, not an action to preserve slavery from the onslaught of Northern abolitionists or the Republican party, not the result of political hysteria; neither was it an escape from the logic of an antithesis between an agrarian South and an emerging Northern industrial order. Instead Johnson argues that secession aimed at protecting slavery and planter rule from other Southerners, most particularly the mass of hill country and piney woods nonslaveholders, whose numbers and alleged coolness to slavery threatened a political and social revolution. To transcend these subterranean social divisions, Georgia secessionists launched a two-pronged revolution in the winter of 1860-61. Secession itself protected the state from the allurements of Lincoln's patronage and prevented the formation of an antislavery Republican party within the state. Then, the state constitution was redrafted to render it a more conservative bulwark against democratic social revisionism at the hands of the hoi polloi.

Johnson's argument is appealing, especially in that it tilts against Eugene Genovese's portrait of a harmonious, organically unified South, where whites and blacks resided contentedly under the aegis of planter hegemony. Unfortunately, *Toward a Patriarchal Republic* is marred by a pattern of intellectual and methodological blunders that weaken the central thesis. For one thing, the book exists in a historical vacuum, with little discussion of pre-1860 events, the structure of politics, and the economic and social character of Georgia. There is a statistical analysis of voting for the secession convention contained within the appendix, yet the main themes of the study nowhere rest upon this data.

More importantly, the author handles his political subjects crudely. He disparages secession as a "folly" unjustified by Lincoln's election. He is convinced that the new president had little power and less inclination to threaten slavery, except by building up a pro-Lincoln constituency within the South. Yet this argument is insensitive to the perception of those involved. Lincoln's election generated real anxiety at that historical moment, a fear, justified or not, that the Republican victory was a terrible symbolic defeat and the signal for a deadly war upon Southern institutions. Then there are patently illogical statements, as when we are told that the proslavery argument was inherently antisecessionist, for if slaves were happy, "separation from the North was unnecessary" (p. 38). The notion that an external enemy might someday undermine the master-slave relation does not trouble the author. Finally, and most fundamentally, Johnson never addresses a basic question raised by his thesis: were secessionist fears of the non-slaveholding class in fact justified? He says a vote against secession (or for the "co-operationist" ticket) was a vote against slavery and the entire structure of antebellum Georgia society. This is wishful thinking in the guise of scholarship.

STEVEN A. CHANNING
University of Kentucky

W. EDWIN HEMPHILL, editor. *The Papers of John C. Calhoun*. Volume 9, 1824-1825. Columbia: University of South Carolina Press. 1976. Pp. lxiii, 682. \$27.50.

With publication of the ninth volume of Calhoun's *Papers*, editor W. Edwin Hemphill completes his eight-volume documentary history of the War Department, 1817-25. This unique series within a series reaches into every corner of War office business and displays the intricacies of managing a small, peace-time military force. Throughout one finds that the introductory essays have excelled in describing military and Indian affairs, but have fallen short in supplying an adequate synthesis of Calhoun's political operations. Repeatedly the essayist instructs his reader that Calhoun was a wise policy-maker and an able administrator, whose labors of office were needlessly burdened by the political opportunism of William H. Crawford, Henry Clay, and John Q. Adams. In this reviewer's opinion the documentary history points to a variant synthesis, which might focus more on issues than personalities.

Monroe's neo-Federal policies of political amalgamation and governmental consolidation posed a serious threat to the survival of Republican Party principles. War and its habits of waste had dis-

placed Jefferson's practice of starving the general government into honest republican simplicity. In the aftermath of the 1819 panic Crawford and his followers complained that the nation had been led to ruin by the seductive lure of easy wealth and false pretensions. Massive frauds within the Second Bank of the United States, large defense expenditures on Monroe's fortress America, protectionist demands from the West for a new American mercantile order, and illegal military operations in Florida further radicalized "old Republicans" in Congress and led them to attack the policies and management of the War and State Departments. Of particular concern to Calhoun were frequent calls for radical retrenchment in the army's budget; other investigations pointed to alleged speculation in the Indian factory system and perversion of the military contracting system.

Calhoun parried radical charges by turning the corruption issue back on Crawford, who allegedly had mismanaged government funds by giving illegal indulgences to favored banks. Out of political jealousy Crawford had induced Congress to retrench military spending by half; no mention need be made that prices had deflated by over fifty percent. Treasury officials had suppressed public documents embarrassing to Crawford's bank dealings. Calhoun took the corruption issue away from Crawford by denouncing "King Caucus," an undemocratic tool of a secret congressional clique. Calhoun and Crawford each ruined the other's bid for the presidency, but the corruption issue survived. After the election Jackson raised his "corrupt bargain" charge, and within a year Calhoun allied himself with those "plain Republicans" whose common identity derived from a shared notion that the general government had been stolen by self-serving interests.

Viewed within a different interpretive framework, Hemphill's documentary history of the War Department proves highly useful in the study of early national politics. In the ninth volume one may track in considerable detail Calhoun's persistent efforts to block Crawford's presidential bid, the War Secretary's rationalization for seeking the vice-presidency, his policy drift toward Indian removal, and his suppression of departmental scandal (see the case of Indian agent John Johnston). Regrettably little documentation on Calhoun's role in the House's presidential selection has survived. Military historians will find more than enough for a season's study, and students of Indian affairs will appreciate the completeness with which that subject is treated. As in previous volumes the editor has selected his documents judiciously and applied his methodology uniformly. Although said in other places, it is worth repeating that no editor ever conducted a more thorough

document search. Subsequent volumes will enjoy the great benefit of having had a first-rate compilation.

WAYNE CUTLER
Vanderbilt University

ARCHIE VERNON HUFF, JR. *Langdon Cheves of South Carolina*. (Tricentennial Studies, number 11.) Columbia: University of South Carolina Press for South Carolina Tricentennial Commission. 1977. Pp. 276. \$14.95.

In this biography Langdon Cheves is represented as a transitional figure in early nineteenth-century South Carolina political and social life. Cheves, son of a loyalist Scots immigrant, was born in western Carolina in 1776. He moved to Charleston, where he attended the Presbyterian church, learned the merchant trade, and decided upon the law as the means to rise above his station in snobish, planter/professional-dominated Charleston. Cheves became one of the most successful lawyers in the city, entered Congress where he supported John C. Calhoun and the War Hawks, married into the aristocratic Dullus family, became president of the Bank of the United States, returned to South Carolina and became a successful rice and cotton planter, trafficked with the unionists during Nullification, became a secessionist, and died in 1857, respected by all of the state. A loyal Carolinian who entered what Archie Vernon Huff calls "the aristocracy of talent," Cheves represented the self-made man of ability and ambition in the antebellum Southeast.

The author divides Cheves' life into three categories: the public figure, the family man who enjoyed the company of the best people, and the successful planter. But the reader is left without an understanding of the interrelationship of these facets of Cheves' life. The background of the embargo, the depression of 1819 and the subsequent turmoil around the Bank of the United States, and Nullification are well described. But how Cheves' political views evolved or which leaders he studied and admired remain mysteries. The biography contains ample commentary on South Carolina's leading families and the intermarriage of its elites but does not attempt to analyze status and social position in the state. Slave life and the problems of a rice planter are discussed in detail, but the actual means to success are ignored, and the economic relationships among the planter elite are overlooked. The reader learns much about the activities of Cheves and almost nothing about personal influences or changes in status and position in South Carolina society.

The documentary material offers evidence of the

behavior and values of the personalities with whom Cheves interacted. Yet the lives of the people around him are neglected. For example, why did his daughter Louisa wait so long to marry, and why did she marry a man much older and less talented than herself? Louisa McCord was one of South Carolina's most brilliant writers, as her essays in the *Southern Quarterly Review* reveal, but little is said about this fascinating woman. As with the other characters in this book, simple comments on events replace any analysis of thought and behavior.

JON L. WAKELYN
Catholic University of America

JAMES L. ROARK. *Masters Without Slaves: Southern Planters in the Civil War and Reconstruction*. New York: W. W. Norton and Company. 1977. Pp. xii, 273. \$12.95.

Deeply researched, superbly written, and winner of the Allan Nevins award, James L. Roark's book is a broad study of planters from the eve of the Civil War through emancipation, as they struggled to maintain both their plantations and slavery. Slavery was the dynamically changing element, moving from strength to collapse and carrying with it, like a dying Siamese twin, the plantation. "So entangled were slavery and the plantation," asserts the author, "that a blow to one was felt immediately by the other."

Before the war planters were committed to the preservation of a distinctive Southern culture which was both realized in and symbolized by the slave plantation. They divided only in that Unionists saw security within the union and secessionists in withdrawal. The threat of invasion by the North welded the two together with impressive ease.

Planter harmony, however, began to erode early in the war. Invasion, bloody defeats, material and social devastation, and roving Northern armies broke the will of many planters to resist. Ironically, the Confederate government itself, initially the very creature of the planters, was a rising threat to planter interests. Impressing slaves, drafting masters and overseers, taxing heavily, pressuring planters to cultivate food instead of cotton, and embargoing cotton, the Confederacy massively violated the sovereignty of the planter over his own plantation. Finally, Davis, like Lincoln, was ready to free the slave to win the war. Finally, too, planters were equally disloyal to the Confederacy as they resisted impressment, evaded service and taxation, and comported profitably with the enemy—especially in cotton. Nonslaveholders, however, did not challenge planter power and, after emancipation, stood firmly by the side of the planter elite.

As divisions multiplied, planters pulled back to the tighter world of their plantations. The key to mastery, they understood, was to control the slave by achieving the isolation of the plantation. Yet Confederate agents, Northern invasions, and emancipation rapidly penetrated the plantation itself. Whenever Union armies came near, slaves departed. There were no bloody rebellions, only disloyal flight. Slavery dissolved "like slabs of earth slipping into a Southern stream."

Profoundly distrustful of free Negro labor, planters worked feverishly. By 1870 they had arrived, reluctantly, at the share tenant system. Caught in a colonial economy and politically defenseless, planter prosperity spiraled downward and the plantation mutated. It was now employer and employee instead of master and slave. Paternalism nearly evaporated. Planters, as they accommodated barely enough to survive, ceased to be what they had been and so did the plantations. "Plantations survived, but plantation life was transformed" because, as a contemporary noted, "the soul is fled."

In carrying the study of planters through these years, Roark has commendably pioneered in a large and almost endlessly complex subject. Most significantly, he cuts across that signal year 1865 when, "almost overnight," the white South was thrown into a rapid flux which would carry it from prosperity to poverty. When slavery dissolved, and the myriad of things slavery implied evaporated, the soul of the Old South had indeed flown. But the body remained, and Southern society somehow stitched itself together. Planters and ex-planters united with nonslaveholders and ex-non-slaveholders, and, yes, finally with black people also. In the South they were all locked together by place and time, and they made their accommodations to one another—sometimes unwillingly, sometimes hatefully, sometimes violently—much more lastingly than with the outside world.

JOEL WILLIAMSON
*Center for Advanced Study in the
Behavioral Sciences
Stanford, California*

SARAH WOOLFOLK WIGGINS. *The Scalawag in Alabama Politics, 1865-1881*. University: University of Alabama Press. 1977. Pp. 220. \$12.00.

The native white Republicans were a crucial element in the politics of Southern Reconstruction. While historians are still unsure exactly who the "scalawag" voters were, they are becoming increasingly knowledgeable about the identity of the leaders—those who held office in the Republican Party. To a large extent, this happy state of affairs

is due to the many articles published by Sarah Woolfolk Wiggins over the last decade. With the appearance of *The Scalawag in Alabama Politics, 1865-1881*, the dissertation—somewhat revised—upon which these articles were based is now available.

The scalawags in Alabama, Wiggins has argued, were numerous and influential in the Republican Party; moreover, they were men of property and of previous political experience. In general, they opposed secession, though rarely the Confederacy as well. Historians are now agreed that, for most of the reconstructed states, such a collective portrait would apply. But *The Scalawag in Alabama Politics* goes beyond this discovery to inquire not only who they were, but what role they played in state and party politics. The answer Wiggins gives to this promising question is, however, disappointing. Having revised the scalawags' social status and political record, she slips back into old ways when evaluating their actions and intentions; for her main interpretive theme is that Alabama scalawags were unceasingly preoccupied with political office. Accordingly, they clamored for patronage and position, creating factional strife with the Northern whites in the party and refusing to accede to the demands of blacks for input and influence. This soon weakened the party so that it became electorally vulnerable and was easily defeated in 1874.

By arguing this point, Wiggins comes dangerously close to concurring with Ryland Randolph, the vituperative editor of the *Tuscaloosa Monitor*, who charged that the scalawag was in the Republican Party solely "to the end that he may step in and be healed of his itch by the ointment of office." Yet to demolish the stereotype of the scalawags' detractors is the avowed aim of the book.

This struggle for office was directly related to the desire of the various groups in the party to direct the policy and determine the composition of this emergent organization. By failing to look behind the mere fight for patronage to see what issues and interests there were, Wiggins naturally jumps to the conclusion that jobs were all that was involved.

Equally questionable is her deduction from this that the Republican Party's survival was jeopardized primarily by the excessive number of positions held and demanded by the scalawags. Their share, she argues, was large, not only in comparison with that of the "carpetbaggers" and blacks but also in proportion to the number of white voters that they actually delivered to the party. Admittedly, blacks were denied reward for their support and this hurt the party, but the Republicans lost in 1874 because they could not win enough votes in the northern white counties. This failure was not because scalawags dominated the party but because, despite

this influence and the assurance it offered, white voters were still not convinced that the Republicans were their party.

Perhaps the obstacle to a more probing evaluation of the scalawags' goals and intentions lies in the organization of Wiggins' book. Rather than being a study of the role of the "scalawag" in Alabama Reconstruction as intended, it is a narrative of Alabama politics in the years 1865-81, with special reference to the scalawags. The result is that the scalawags cease to be the protagonists in the drama; while their actions are recorded, they are never scrutinized and analyzed carefully. And this is a shame because the inquiry upon which Wiggins embarked is so much more important than overthrowing old stereotypes.

MICHAEL PERMAN
University of Illinois,
Chicago Circle

EDWARD C. WILLIAMSON. *Florida Politics in the Gilded Age, 1877-1893*. Gainesville: University Presses of Florida. 1976. Pp. x, 234. \$8.00.

According to Edward C. Williamson, the pattern of present-day Florida politics was formed during the years 1877-93. In this excellent study of politics and land deals in what was then a poor and sparsely settled state and the southern frontier of the United States, Williamson has been successful in researching and arguing the thesis he has set forth. His book is detailed, carefully based on a variety of sources, and well written. It moves along at a good pace, and what could have been a dull study is a lively and an arresting one. Both in his narrative and analysis, Williamson makes a solid contribution to both Florida and Gilded Age politics.

As Williamson sees it, when the Democratic Party in Florida returned to power at the end of Reconstruction, its legislative leadership was largely comprised of white county leaders who, because of the relative isolation of much of Florida, were not answerable to a state-wide political machine or to political leaders with large state-wide followings. How the Bourbon governors of this era dealt with the problems facing them shaped the political patterns of contemporary Florida. To open South Florida and the panhandle it was necessary to have railroads, and Republicans and Democrats alike offered public land at low prices as an inducement for railroad building. The purchase of four million acres of Florida public domain by Hamilton Disston, a wealthy Philadelphia industrialist, for twenty-five cents an acre announced an influx of northern investment capital.

While conservative or Bourbon Democrats

largely endorsed such a move, a group of young, idealistic Florida whites, organizing an Independent movement, opposed it. The Independents were also disillusioned by the election frauds and lawlessness condoned by the Bourbons and by their failure to protect the civil and political rights of Negroes. But the Independents, in their efforts to defeat the Democrats in 1884, were unable to win a single county in South Florida, the area with the largest number of former northerners, and this cost them the election. The Independent revolt marked the last major attempt to involve Negroes in Florida politics until the New Deal. Later on, Florida's Populists, withdrawing from a coalition with railroad interests made by the Alliance, sought to challenge the now almost impregnable position of the Democrats. But Populism's failure to gain substantial support and its resulting defeat left the Democratic Party without a major rival in the state. The Populists now followed the Independents and Republicans into defeat and political oblivion.

This readable book should appeal to scholars, students, and general readers alike.

VINCENT P. DESANTIS
University of Notre Dame

ROBERT M. UTLEY, editor. *Life in Custer's Cavalry: Diaries and Letters of Albert and Jennie Barnitz, 1867-1868*. (Yale Western Americana Series, number 30.) New Haven: Yale University Press. 1977. Pp. xiv, 302. \$15.00.

Albert Barnitz, a Pennsylvania native who grew up in Ohio, published a volume of poetry in 1857 when he was twenty-two years old. During the Civil War he served as a captain of cavalry under Custer and Sheridan, and was twice brevetted for gallantry in the war's closing campaigns. After a little more than a year in civilian life, Barnitz in November 1866 was appointed captain in the newly created Seventh Cavalry Regiment. On February 11, 1867 he married Jennie Platt, a popular belle of Cleveland, Ohio, and ten days later bride and groom settled in quarters at Fort Riley, Kansas. Their letters and journals for the next twenty-one months comprise the greater part of *Life in Custer's Cavalry*. Because both Barnitz and his wife were perceptive, articulate people, and because his life as an Indian fighter was always interesting and sometimes exciting, their writings make absorbing and enlightening reading. Robert Utley's editorial commentaries and documentation tell the reader all he needs to know about persons, places, and events mentioned in the documents.

Since most of Barnitz's frontier service was de-

voted to active campaigning, Jennie was with him for only a few months. Their longest sojourn together, which she thoroughly enjoyed, was at Fort Leavenworth in the winter of 1867-68. When back in Cleveland she suffered much from anxiety about her handsome husband, and she had ample cause for apprehension. Disease, exposure, and combat made his life precarious.

Barnitz endured the perils of soldiering with equanimity. His principal complaints were about his associates, most of whom drank to excess. He considered West Point training to be more of a handicap than an advantage for frontier service. He had a high regard for Sheridan, but he had little use for Custer, his regimental commander, whom he criticized for incompetence, neglect of duty, and excessively harsh discipline. Brutal treatment he deemed largely responsible for the high rate of desertion among enlisted men. Another of Barnitz's grievances concerned the government's policy of giving arms (later used to shoot whites) to Indians to make them accept treaties that they did not understand. Barnitz denounced the Indians for their atrocities but praised them for their bravery. His service against them was terminated by a near-fatal wound he received in the battle of Washita, November 26, 1868. He received a disability discharge in November 1870 and devoted his remaining forty-two years largely to travel.

BELL I. WILEY
Emory University

WILLIAM T. HAGAN. *United States-Comanche Relations: The Reservation Years*. (Yale Western Americana Series, number 28.) New Haven: Yale University Press. 1976. Pp. xvi, 336. \$19.50.

This is a study of Comanche, Kiowa, and Kiowa-Apache relations with the United States from the middle of the nineteenth century until 1910. These fierce tribes were a menace to the state of Texas and to the Mexican settlements of the Southwest when a series of treaties culminating with the Treaty of Medicine Lodge in 1867 reduced their broad domain to a "permanent" reserve lying between the Red and Washita rivers in Indian Territory. While it was the intention of the federal government to prepare these tribesmen for assimilation through the influence of missionaries and teachers, native culture proved a nearly insuperable obstacle. The Quaker Superintendent Enoch Hoag is regarded as being naive in believing that these Indians would reform in response to councils, lectures, and threats to withdraw rations or annuities. Although Congress did not support assimilation efforts with sufficient funding, the

judgment of William T. Hagan that Quakers were ill equipped to cope with warlike tribes seems well established. As members of one band remarked, they would "give up raiding when the white men gave up paper and books." While military officers were also somewhat unrealistic, as in the matter of refusing to ransom white captives, they are here regarded as being better suited to manage the Comanche, Kiowa, and Kiowa-Apaches than were civilian agents. Because military force was necessary to suppress habits that were endemic in the culture of nomadic warriors, the treaties failed to bring peace to the southern plains. They did, however, "give the stamp of legitimacy to United States efforts to concentrate the Indians and open the region to white exploitation."

While assimilation as well as confinement was a main objective of the Treaty of Medicine Lodge, the book amply illustrates that the result was landlessness and the erosion of native culture. A great deal of attention is given to the Jerome Agreement of 1892 which the Comanches, Kiowas, and Kiowa-Apaches were pressured into signing as an alternative to the provisions of the Dawes Act of 1887. Eighteen whites were declared eligible to share in the benefits of this agreement, undoubtedly as a reward for serving as persuaders in securing the 456 signatures that represented more than three-fourths of the adult male Indians. Yet it is acknowledged that the agreement would have had no chance of being accepted without the support of Quanah Parker. The wily Comanche chief was motivated by a pecuniary self-interest in grass leases to Texas cattlemen, and it was to these same cattlemen that Quanah turned for support in delaying ratification of the agreement by Congress. The cattlemen and the tribes had a mutual interest in preventing the reservation from being broken up and parceled out in 160-acre homesteads. When ratification finally came in 1900, the tribes continued to hold 480,000 acres in common—although this was the least desirable portion of the reserve created in 1867. Even this tract was opened to settlement in 1906, although President Theodore Roosevelt did use the threat of veto in order to secure allotments for Indian children born after 1900 and to raise the compensation for the surplus land from \$1.50 to \$5.00 per acre.

In many respects the book may be regarded as a case study of the effort to confine and assimilate Plains Indians under the "Peace Policy" and the land allotment system. Plains Indian culture and the unsuitability of the natural environment for agricultural homesteads doomed these efforts to failure. The Comanches and other tribes commonly hired white farmers to work their allotments, usually on a sharecrop basis. By one estimate ninety percent of the land to which they

received full and independent title was promptly sold and much of the proceeds was squandered. The military agents who served after 1893 are seen as being an improvement over the civilians who served at Anadarko earlier. Agent Frank Baldwin was the best of them and incurred the enmity of the adjacent white community and every other group that favored ratification of the Jerome Agreement. But despite support from the Indian Rights Association and Senator O. H. Platt, he could not cope with railroad lobbyists and congressmen whose political interests were adverse to tribal autonomy.

HENRY E. FRITZ
St. Olaf College

ROBERT ELMAN. *First in the Field: America's Pioneering Naturalists*. New York: Mason/Charter. 1977. Pp. xx, 231. \$12.50.

When I first examined this book I hoped that it would be a superior specimen of the popular history so badly needed today. This expectation was based on Robert Elman's correct spelling of W. J. McGee's name, which seemed to indicate the requisite care in research. I was somewhat disappointed, but not totally.

Written out of the contemporary concern with lessening natural resources and an increased ecological awareness, Elman's study concentrates on the development of natural history in America. His naturalists are all well known: comparative zoologist and geologist Louis Agassiz; John Wesley Powell, explorer of the Grand Canyon and archetypical scientist-bureaucrat cut to the generous pattern of the Gilded Age; artist James J. Audubon; nature writer and friend of presidents John Burroughs; pre-Audubon bird and animal painters Mark Catesby and Alexander Wilson; and, lastly, the Bartrams, father and son, who botanized through colonial America.

The lives are comprehensive. Elman covers disappointments, poverty in some cases, struggles for recognition, bickerings, and petty rivalries, as well as successful efforts to contribute to the natural history of North America. His use of a narrow biographical approach to his subject, however, necessitates slighting the intellectual and institutional aspects of the development of natural history. This will lessen the value of the book for scholars and may misrepresent the subject to laymen. His writing sometimes takes on a "Whigish" tone that dismisses those who were not in the mainstream of scientific truth and repetitiously praises those who were, while patronizingly making excuses for the latter when an unfortunate lapse into error does occur.

There is little on the idea of nature or of science in the early naturalists. Catesby and the Bartrams travel about collecting, naming, and drawing plants and animals, but to no clearly delineated purpose beyond discovery itself. The strange and fascinating Scot Alexander Wilson seems to be a favorite of Elman's, perhaps because of his unconventional ways and early radicalism, but he, too, simply travels, draws, and classifies. Elman seems to assume that science is its own justification. Agassiz's theoretical side, without an understanding of which his approach to natural history is inexplicable, receives little attention. His ice-age theory is described, but the reader is left in the dark as to why he believed it; his anti-evolution views are treated as quaint. Elman's discussion of Powell's theory of canyon formation could easily leave the impression that Powell discovered the individual processes of geological uplift, erosion, and sedimentation. Finally, the book contains occasional anachronisms.

On the positive side, the sketches are very well written and make for pleasant reading. They will serve well to introduce the personalities with whom Elman deals, and to convey a sense of their interest in nature.

NEAL C. GILLESPIE
Georgia State University

ELIZABETH STEVENSON. *Park Maker: A Life of Frederick Law Olmsted*. New York: Macmillan Publishing Company. 1977. Pp. xxv, 484. \$17.95.

The figure of Frederick Law Olmsted is regaining the heroic proportions it assumed to nineteenth-century Americans. The career that encompassed many careers—social critic, student of the antebellum South, secretary of the United States Sanitary Commission, president of the first Yosemite Commission, planner of Central Park, Prospect Park, and park systems in Boston, Detroit, Rochester, Buffalo, and Chicago, landscapist for the U.S. Capitol, Stanford University, and the Columbian Exposition—has struck a responsive chord in our environmentally conscious generation. Thus Elizabeth Stevenson's fascinating biography joins recent work by Albert Fein, Laura Wood Roper, and Charles McLaughlin, in assessing Olmsted's giant contributions.

It is, of course, difficult to be brief about Olmsted; this biography, like Laura Wood Roper's, is massive; and the edition of Olmsted's *Papers* will overflow into a number of volumes. But its size is held together by the counterpoint Stevenson creates between the landscape architect's life and his achievements. Impulsive, irascible, moody, feverishly active, Olmsted endured a personal life

marred by a series of domestic tragedies and dogged by recurring bouts of bad health. A partial invalid after a carriage accident in Central Park, he could never make an easy adjustment either to life or his own great abilities. The spells of nervous anxiety and depression eventuated in a twilight period of senility that consumed the last half-dozen years of his life. How fitting, if ironic, his search for landscapes that restored a sense of peace, harmony, and tranquility to the fevered lives of urbanites, his insistence upon screening out the noise and distractions of city life. In his preference for the natural beauty of the English landscape and his aversion to elaborate decoration and formal planning Olmsted seemed to be offering a prescription for himself as well as his contemporaries.

The Stevenson book, based on long and careful research, and supported by appropriate citations, is evocative and absorbing. Obviously, choices had to be made. Students concerned with the evolution of landscaping techniques, and with some of the social and political implications of Olmsted's views, will have to turn to the Olmsted *Papers* and to secondary sources. Many major projects are introduced and then dismissed in a paragraph or two. And there is little indication that Olmsted's outlook on landscaping changed significantly during the four decades of his practice. Only some hints here and there suggest that the Olmsted planning Vanderbilt's Biltmore was not entirely the same designer who, with Calvert Vaux, proposed the Greensward Plan for Central Park in the 1850s. But in place of extended comments about landscape architecture as such, one gets what Stevenson planned: a complex yet coherent portrait of the man, which manages to be fair and generous at the same time. It will introduce readers, easily and authoritatively, to one of the original geniuses of our culture, and Olmsted devotees will find much to learn and consider as well.

NEIL HARRIS
University of Chicago

HENRY K. BEECHER and MARK D. ALTSCHULE. *Medicine at Harvard: The First Three Hundred Years*. Hanover, N.H.: University Press of New England. 1977. Pp. xiv, 587. \$27.50.

It is often said that from the 1630s, when it was the first American college, Harvard has been synonymous with excellence in education. In this book, Mark D. Altschule and the late Henry K. Beecher support that generalization with a lengthy description of Harvard's contributions to medical science. An impressive parade of physicians, scientists, and

educators is marched past the reader, collectively representing a who's who of American medical science from the late eighteenth century to well into the twentieth century. With such an illustrious cast of characters, the authors have no trouble portraying Harvard as the medical capital of America during the nineteenth century, and of the world during the twentieth century.

Supporters of other institutions have put forward similar claims, most notably those of the University of Pennsylvania and Johns Hopkins. The authors try to "correct" the record by taking issue with the supporters of Pennsylvania and Hopkins, pointing out that Harvard was first to do this and first to do that, and that the situation in Boston was superior to that in Philadelphia or Baltimore. These often contradictory claims have resulted in a running debate over the years, a debate that never seems to end. Clearly parochialism should be cast aside, and the historian should strive for more objectivity and a broader perspective. Parochialism often leads to inconsistencies, and this book provides an excellent example of that problem. After providing glowing descriptions of the scientific accomplishments of the faculty from 1782 to 1869, for instance, the authors confess that the school was "a money-making institution not much better than a diploma mill" until President Charles W. Eliot converted the "diploma mill" into an example of excellence in medical education (p. 87).

Yet, despite the parochial approach, this book can be useful to those seeking to understand several major developments in American medical history. The influence of French medicine, for example, persisted at Harvard long after the German influence had replaced the French in the 1870s and 1880s. Thus, with its emphasis on the benefits of the German system, Abraham Flexner's celebrated report failed to evaluate Harvard in the correct context. Harvard disregarded the Flexner Report and its criticisms, insisting that the report was not relevant to an institution that was university controlled, had high admission standards, and had plenty of clinical material. The authors are critical of Flexner's approach, exclaiming that "today's medicine, which many find irrelevant to patients' needs, is the fruit of Flexner's report," and Flexner himself is dismissed as "a well informed but short-sighted reformer" (pp. 183-84). In addition, there is a brief but enlightening chapter on blacks, women, and Jews in the student body.

Finally, it should be noted that the authors provide a gold-mine of information on the history of medical science at Harvard, and that in itself represents a major contribution to the literature of the field.

MARTIN KAUFMAN
Westfield State College

THOMAS L. HASKELL. *The Emergence of Professional Social Science: The American Social Science Association and the Nineteenth-Century Crisis of Authority*. Urbana: University of Illinois Press. 1977. Pp. xii, 276. \$12.00.

Professional social science as we recognize it today did not exist in the United States in the 1860s, but it had become an established, if controversial, presence by the first decade of the twentieth century. Thomas L. Haskell's important book takes for granted that the social sciences developed in imitation of the natural sciences but seeks to explore the theoretical and institutional means by which this imitation increased so dramatically in scope and intensity when it did. Haskell proceeds on two levels. One is a detailed intellectual, social, and institutional history of the American Social Science Association, the organization that spawned—prior to its own demise near the turn of the century—most of the major professional organizations in the social sciences, including the American Economic Association, the American Historical Association, and the American Political Science Association. The second level of Haskell's inquiry takes form as an interpretive essay on the transformation of social theory in Western societies, especially in the 1890s.

Haskell's history of the American Social Science Association is in every respect an able piece of research and analysis. He traces the rise of the organization to a "crisis in professional authority" felt by American elites after mid-century. What will justifiably attract attention to this book, however, is Haskell's use of this organization's death to illustrate the plausibility of his interpretation of the transformation of social theory. That interpretation builds effectively upon the works of a generation of historians (particularly Morton White, H. Stuart Hughes, J. D. Y. Peel, Philip Abrams, and Robert Wiebe), but focuses imaginatively on the problem of causal attribution. In the perspective of this problem, Haskell shows that the various "revolts" and "movements" of the epoch shared a vivid sense of the "interdependence" of social phenomena and a consequent conviction that such phenomena could be adequately explained only by attention to a chain of antecedent and contemporary "links" too lengthy for any but a full-time, "professional," "scientific" inquirer to follow.

A less vivid sense of interdependence and a less firm conviction about the length of causal chains rendered most members of the American Social Science Association "old hat" by the 1890s; these older, proto-professionals continued to believe that social phenomena could be explained by enlightened amateurs focusing on aspects of experience that their successors regarded as surface phenom-

ena, as mere "symptoms" instead of causes. The most salient of the contested issues was the status of "intention" in individuals: members of ASSA were still inclined to attribute independent causal efficacy to the will of individuals, while to the generation of the 1890s the exercise of this individual will was much more "dependent" on other conditions.

Haskell explicitly and correctly acknowledges that no study of a single organization—especially one that died—based in but one of the several national cultures in which social theory was transformed can "prove" the validity of any interpretation of the transformation. Yet the plausibility of Haskell's argument is strikingly advanced by his chosen "case," and his commentaries on the implications of the work of other scholars are incisive and subtle. A fascinating implication of Haskell's book is that if one rejects its fundamental points, so too must one reject the most widely accepted claims of many of the earlier scholars upon whom he has relied. One example of this situation will suffice. The notions of "cultural organicism" and "historicism" are crucial to, but only briefly developed in Morton White's 1949 classic, *Social Thought in America*. Haskell elaborates upon both of these ideas, and argues that they are essentially the synchronic and diachronic extensions of the notion of "interdependence" around which Haskell's own interpretation revolves.

Another theme of this ambitious work is Haskell's attempt to add a social dimension to our understanding of the appeal social science "experts" had for educated Americans in general. Here, Haskell points to the changes produced by urbanization, industrialization, and the revolutions in transportation and communication. He suggests that the "complexity" historians conventionally trace to these processes entailed something more specific: "interdependence" as an objective fact in social life. If it was, indeed, more difficult for people to believe that their own lives were explicable in terms of their own volitions and those of their neighbors, it is not so surprising that "experts" in social explanation got a hearing.

During the past decade a number of helpful works have been written about the early history of the social sciences in the United States. Of these, some are more successfully integrated and more complete in their coverage of a prescribed area than is Haskell's double-barreled presentation, but none has contributed more than Haskell's book now does to our effort to understand the origins, the sustaining conditions, and the early consequences of professional social science in America.

DAVID A. HOLLINGER
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VIRGINIA YANS-MCLAUGHLIN. *Family and Community: Italian Immigrants in Buffalo, 1880-1930*. Ithaca, N.Y.: Cornell University Press. 1977. Pp. 286. \$12.50.

Virginia Yans-McLaughlin describes her intentions too modestly in her introduction and conclusion to *Family and Community: Italian Immigrants in Buffalo, 1880-1930*. She tells us that this study serves as a tentative beginning in the examination of how immigrant families retained and modified their traditional culture during the process of adjustment to urban industrial America, because it concentrates on one ethnic group in one community. She notes patterns of Italian-American life and suggests possible analyses. She also sets forth her methodology cautiously. It combines the statistical with the qualitative. Using printed sources, manuscript censuses, and oral history interviews, she offers an impressionistic overview of Italians in Buffalo. Perhaps this defensiveness arises from a concern that the urban quantifiers will scoff at an in-depth case-study that makes no claim to interpret the larger questions of continuity versus disorientation, or that a social scientist will see weakness lurking in the absence of multiple regressions and fertility chart age-of-marriage diagnosis of cultural change.

This study goes beyond the author's modest claims by drawing upon a broad range of materials which include newspapers, government records, manuscript censuses, and church records to describe the Italian community in Buffalo. She incorporates a multi-disciplinary approach using the anthropological, sociological, psychological, and demographic literature which relates to the subject. Through the use of foreign-language materials and oral interviews, she looks into the lives of the women and men who made the transition from a preindustrial culture to an urban environment. This method adds a human dimension to the story of people facing the problems of high infant mortality rates, irregular, low-paying employment, and threats to family cohesiveness. Real people, not neutral numbers, work, marry, raise children, join organizations, buy houses, and dream of the future in these pages.

Yans-McLaughlin posits a dialectical model of social change in which the family's flexibility relies upon traditional forms and ways of relating, while adapting to new socioeconomic conditions. She reviews the south Italian experience of the families of laborers who dominate Buffalo's immigrant community. She notes the patterns of underemployment for men and the tendency of married women to remain at home. She describes the family as the major control factor in a society that kept the church, government, and economic systems outside the kin structure. She follows these families

to Buffalo and explains how they tended to perpetuate these practices—the men preferring outside construction work to more stable heavy industry, the women venturing out of the home to work only under very special conditions that ensured contact with kin and isolation from corrupting influences. She compares the Italians to the Polish immigrants in Buffalo and suggests that the latter's different response to the economic setting supports a thesis of cultural influence.

The author deserves praise for her efforts to grapple with the important issues of cultural continuity and adaptation, and class as opposed to cultural influence upon behavior. Yet within the careful limits of her study a number of questions remain unasked or underdeveloped. She does not define how the working-class characteristics only partly explain immigrant behavior. She does not consider the experience of unmarried daughters in southern Italy and in America. She does not trace development over time, the 1905 cohort in 1925. (We do not know for example the chronological period that her interviews cover.) She neglects the crucial factor of family budgeting, both in Italy and in America, which enables women to remain at home. She uses the oral history technique only to obtain attitudinal and family values data, not also to expand upon the written record. And she fails to provide a sketch of the urban landscape of Buffalo, its population, and economic features.

Yans-McLaughlin does herself a disservice by neglecting to explain the method she uses to obtain her quantitative information and, more important, to detail her successful technique of integrating the qualitative and quantitative. The continuity and adaption she posits continues into the 1920s as "the unskilled and uneducated sons of immigrants repeated their fathers' careers in building, general labor, and service occupations." Although she explains the earlier relationship between Old World traits and New World options, she neglects to explain how these are retained over time as the conditions and opportunities of the new environment bombard the traditional culture.

JEAN SCARPACI
Towson State University

CHARLES VAN RAVENSWAAY. *The Arts and Architecture of German Settlements in Missouri: A Survey of a Vanishing Culture*. Columbia: University of Missouri Press. 1977. Pp. xvi, 533. \$45.00.

Charles van Ravenswaay's massive volume is the landmark result of a unique pilot study of nineteenth century German culture in eight Lower Missouri Valley counties. Sponsored by the Archives of American Art in 1961, this study has been

eminently successful in developing techniques for investigating, documenting, and describing historic immigrant culture; it is a model for future ethnic research and local history publications. The approach is intensely personal, focusing on biographical detail of early settlers, craftsmen, and builders. Van Ravenswaay combines the nostalgic appreciation of a native son with the critical perspective of the professional historian. In the 1930s, prior to his directing the Missouri Historical Society, Old Sturbridge Village, and the Henry Francis du Pont Winterthur Museum, he began conducting the neighborly interviews and identifying and photographically recording the regional art, crafts, and architecture that, together with published memoirs, county histories, state gazetteers, census reports, and applications for citizenship, became the sources for this monumental effort.

German cultural influence upon America is proverbial, but little attention heretofore has been given to the thousands who emigrated from every German province to land surrounding St. Louis after 1832. Gottfried Duden's *Report of a Journey to the Western States of North America* (1829) helped lure Germans from every social, intellectual, and economic rank of the upper middle to the peasant classes. Barons, counts, engineers, preachers, and artists founded the aristocratic "Latin Settlement" of the Berlin Emigration Society in the early 1830s. Professional men, merchants, artisans, and increasing numbers of farmers and peasants immigrated later with Frederick Steines' Solingen Society and the Giessen Society led by attorney Paul Follenius and Lutheran minister Friedrich Muench who dreamed of creating "a German model republic" (p. 38). Before immigration declined in the mid-1850s, the religious, linguistic, and class differences that so effectively divided the Missouri Germans in the fatherland created tensions and forced adjustments in the freer atmosphere of the American backwoods where "many a fellow, whom one considered incapable of even expressing an opinion about a hatching hen in Germany, here believes himself called upon to pass judgement on the actions of others" (p. 13).

Because many came to America to preserve a way of life that was disintegrating in Europe, their communities and crafts reflect Old World traditions in reluctant compromise with the New World environment. Unfamiliar climate, trees, plants, and animals required innovations. Despite such adjustments, German folkways, faith, music, arts, and architecture were stamped upon the Missouri countryside, although not so indelibly, fears van Ravenswaay, that they could not yet be lost.

Wedding historical narrative to physical culture, the author achieves the study's second intent, to

examine and identify the contributions of the Missouri Germans to the architecture, arts, and crafts of the United States in the 1830-60 period. More than six hundred carefully identified photographs and drawings, including twenty color plates, and four hundred pages of analytical text represent this contribution. Log, frame, stone, and brick construction with the designers and builders are discussed in minute detail. Forty percent of the book is devoted to craftsmen and their furniture, musical instruments, wood and stone carving, stone-cutting, baskets, firearms, tin and copperware, textiles, pottery, drawings, prints, paintings, and trades. Such comprehensive treatment marks the beginning of a welcome new era in ethnic histories.

DONALD E. PITZER
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Evansville

HAL D. SEARS. *The Sex Radicals: Free Love in High Victorian America*. Lawrence: Regents Press of Kansas. 1977. Pp. xi, 342. \$15.00.

The origins of the "free love" movement in the United States are closely related to the growth of feminism and spiritualism, a mixture of infidelities which was often enriched by the addition of bits of abolition, phrenology, and hydropathy. Spiritualism promised liberation from conventional religion; the free love movement offered release from sexual and familial constrictions. Both causes provided a model of feminine power and leadership. To its traditional defense of women's rights and religious freedom, the free love cause after the Civil War added the struggle for birth control, sex education, and a free press. In *The Sex Radicals*, Hal Sears defines the nature of that threat to morality posed by the feminists, freethinkers, and anarchists who had buried their differences enough by the 1870s to present virtually the only organized resistance to the Comstock-inspired attack on civil liberties.

But these defiant libertarians of the late Victorian period were not libertines. "Free love" rarely meant absolute sexual freedom to its advocates, but simply that there should be no legal restrictions on sexual behavior. The sex radicals frequently espoused "Victorian" beliefs such as coitus only for procreation and the moral superiority of pure womanhood. Like the Comstockers who opposed them, most sex radicals feared orgasmic sexuality as an awesome force which demanded control.

It was the free-lovers' faith in *self-control* that made anarchistic individualism seem possible to them and that set them apart from the Social Purists who sought state regulation of sensual de-

lights. Similarly, although sex radicals shared with suffragists a belief in feminist solutions to social problems, they were not a subsidiary of the more respectable nineteenth-century feminist movement. To sex militants, woman-power was centered in the modified bedroom, not at the polling place. Sex radicals agreed that emancipation for both sexes could come only after the abolition of the traditional institution of marriage.

Sears focuses upon two groups of love reformers, one clustered about Moses Harman, whose journal, *Lucifer, The Light Bearer*, united sex radicals from 1883 to 1907, and the other around Ezra and Angela Heywood and their Massachusetts paper, *The Word*. Fond of contention, these dissidents jolted public morality by their frank discussions of oral sex, homosexuality, and the sexual abuse of women. The singular Angela Heywood defended the free use of four-letter words; Lois Waisbrooker, one of a series of remarkable women editors of *Lucifer*, gleefully demanded the arrest of the secretary of agriculture under the Comstock Act for his release of a government book on swollen horse ailments, in the notorious Horse Penis Affair of 1892; meanwhile, the wealthy Doctors E. B. Foote, father and son, promoted contraception, courted jail, and generously financed the legal defense of free-lovers and anarchists.

Sex radicals paid for their defiance of the law—with prison terms, heavy fines, and at least fifteen suicides. Their history is an important contribution to an understanding of the complex shift in sex relations which followed in the wake of western industrialization.

DEE GARRISON
Rutgers University

ROBERTA FRANKFORT. *Collegiate Women: Domesticity and Career in Turn-of-the-Century America*. (New York University Series in Education and Socialization in American History.) New York: New York University Press. 1977. Pp. xix, 121. \$10.95.

Collegiate Women is concerned with attitudes toward careers versus domesticity in America from 1890 to 1910 among a highly selective sample of female educators and students. The author has chosen Elizabeth P. Peabody, Alice Freeman Palmer, M. Carey Thomas, and Ellen Richards, and materials from alumnae registers at Wellesley and Bryn Mawr to illustrate effects of conflicts between traditional educational ideals of the passive, nurturant female and new, college-inspired goals of Wellesley and Bryn Mawr for alert, sophisticated, and even economically independent women. Roberta Frankfort discusses and contrasts skillfully the styles and educational objectives of Presidents

Thomas at Bryn Mawr and Freeman at Wellesley. Thomas was an uncompromising feminist who hoped her graduates would make a name for Bryn Mawr which would rival Oxford. When she was in complete command (1894–1910), intellectual standards were rigorous, including a compulsory oral examination by the president before a student's graduation. Thomas' personal style was abstemious and seemingly hostile to men. Alice Freeman at Wellesley was more traditionally feminine in both personal life and educational influence. She believed that "learning alone is not enough for women" and educated to refine "character, sympathies and moral sensibilities." Wellesley students, unlike Bryn Mawr's, tidied up their own dormitory suites. President Freeman's decision to renounce her position and leave Wellesley because she outranked her husband, George H. Palmer, a mere professor at Harvard, illustrates her great sensitivity to preserving "feminine" identity.

The author uses alumnae registers to show that about 60 percent of Bryn Mawr graduates remained unmarried in the 1890s, while almost 15 percent went into college teaching. Of those that married about one-half had only one child or no children at all. Wellesley's graduates showed less independence than Bryn Mawr's, but they were also "liberated" by comparison with national norms. By 1910 the assertive ideal for women receded as new outlets in home management, nursing, and social-work helped restore a nurturing ideal.

This book is pleasant and informative but has numerous flaws. Its title is presumptuous; the lives of four women educators and statistical samples from the graduates of two colleges does not cover the ground. If the author had used college newspapers as a source she might have fared better. She stresses the impact of colleges on women's attitudes but underestimates the influence of magazines and emancipatory morals in cities. Why the retreat from the Bryn Mawr ideal? How many college teaching positions for women were available after all? To escape the difficulties of this type of economy feminine wiles were still essential.

JAMES R. MCGOVERN
University of West Florida

EMMA SEIFRIT WEIGLEY. *Sarah Tyson Rorer: The Nation's Instructress in Dietetics and Cookery*. (Memoirs of American Philosophical Society, number 119.) Philadelphia: The Society. 1977. Pp. ix, 196. \$6.00.

Interest in the history of women first brought from obscurity the stories of reformers and militant crusaders for equality. Subsequent scholarship is

bringing to prominence influential women whose platforms were less political.

Such a subject is Sarah Tyson Rorer, whose platform was literally the kitchen on stage. From her first small cooking class taught in 1880 at the New Century Club of Philadelphia, she built a national reputation as dietitian, home economist, and kitchen consultant to the nation. Her career included founding and directing the Philadelphia Cooking School, presiding over model kitchens at the 1893 Columbian Exposition in Chicago and the 1903 Louisiana Purchase Exposition in St. Louis, operating the Rorer Restaurant in New York, and participating in the Pennsylvania Chautauqua program. She was an active touring lecturer and served as columnist for several publications, including *Table Talk* and *Ladies' Home Journal*. At least one of the fifty-four cookbooks or booklets she produced probably graced the kitchen of most turn-of-the-century homes. She was well enough known as a symbol of kitchen management that Jerome Kern and P. G. Wodehouse included a song entitled "Mr. and Mrs. Rorer" in their 1924 musical *Sitting Pretty*, depicting her culinary craftsmanship as a marital cure-all.

Part of the appeal of the "Mrs. Rorer's Kitchen" demonstrations and writings was her terse, sometimes caustic, epigrammatic wit. Her common-sense preachments, however, of balanced individual diets, full use of food to avoid waste in a hungry world, and well-planned home management brought the field of home economics close to the status of homemaking science. Though she herself was a transition figure in the era when household arts were becoming the professions of home economics and dietetics, her pupils would be founders of such organizations as the American Home Economics Association.

Though a prolific publisher, Sarah Rorer left no body of manuscripts to aid her biographer. Emma Weigley's study is, therefore, primarily a compilation of material from Rorer's publications and newspaper accounts of her appearances, supplemented by sparse information from interviews and contemporary biographical guides. To appreciate it fully the reader must savor such Rorerisms as, "Pie is the foundation of dyspepsia" (p. 77), and should enjoy learning that "hominy grits, made into a custard and frozen with the addition of whipped cream and a compote of fruit, is one of the most elegant desserts we have ever tried" (p. 95).

The author has drawn a verbal portrait of a strong-willed, sharp-witted woman whose showmanship and expertise in dietetics and cookery moved her from her own family hearth into the profession of public home economist. Sketch by sketch, Weigley uses an almost scrapbook style of

history to identify her subject as a public symbol and to reveal at least a profile of the private Mrs. Rorer. Additional studies of this type would enrich American social history.

JAMES HOWELL SMITH
Wake Forest University

AKIRA IRIYE. *From Nationalism to Internationalism: U.S. Foreign Policy to 1914*. (Foreign Policies of the Great Powers.) Boston: Routledge and Kegan Paul, 1977. Pp. viii, 368. \$14.00.

In his sixth book Akira Iriye considers American foreign relations from the Civil War to the First World War, and like so many historians in recent times he refuses to lay it out in narrative form and seeks to produce an interpretive survey. At the very beginning he remarks two other interpretations of the foreign policy of the United States—the realism versus idealism school, and the continuity and unity school. Those interpretations he rejects gently, saying that regardless of their merit or demerit they “do not seem totally adequate as analytical tools when one studies United States foreign relations in a comparative framework and in an international context, which is the aim of this book.” The author prefers five “levels or modes” of American foreign relations: geopolitical factors, internationalist ideas, national interest considerations, special interests, mass culture. By using these components or levels he hopes to make more sense out of the moves and countermoves, the demarches, the hopes and successes and failures, of foreign policy in the latter nineteenth century.

The author certainly is in control of his subject. The organization is careful: after an introduction giving details of the five levels or modes of American attitudes there are chapters on European imperialism and American expansionism, the growth of nationalistic expansionism, the Spanish-American War and its aftermath, and the subsequent decade or so (of rapprochement with Britain, control in Latin America, the open door in the Far East, and threats of war in Europe). The writing is smooth, the narrative—attenuated though it is—interesting. Occasionally the author makes a factual slip, as in describing William McKinley and William Jennings Bryan as senators, George Dewey before Manila Bay as an admiral, Francis M. Huntington Wilson as Fred M. Huntington Wilson. The slips are not numerous, and taken altogether this is an admirable account.

Where a reviewer will raise questions is, inevitably, over what Iriye describes as his perceptions, his analytical tools or levels or modes of analysis. They are not nearly as novel as he believes; almost any American diplomatic historian uses several or

all of them. Moreover, the writing of American diplomatic history according to a set of generalizations has not produced many books of lasting scholarship. Cold War revisionism seems to be going out of fashion as quickly as it came in. It is worthwhile remarking that the two great general historians of American history in our time, whose books will surely last for years to come, Allan Nevins and Samuel Eliot Morison, were both narrative historians. Diplomatic historians may be forced to admit that narrative does explain what happened and, thereby, if done with sufficient deftness to avoid flooding the reader's mind with details, why it happened.

ROBERT H. FERRELL
Indiana University,
Bloomington

DAVID MCCULLOUGH. *The Path Between the Seas: The Creation of the Panama Canal, 1870-1914*. New York: Simon and Schuster, 1977. Pp. 698. \$14.95.

David McCullough is one of those authors who sees history as an absorbing story, holding one's attention like a James Bond novel. Emphasis is on heroic accomplishments and exotic enterprising adventures. Yet McCullough has the skill to treat majestic subjects with scholarly acumen. He has already demonstrated this talent in his treatment of the Johnstown flood and the building of the Brooklyn Bridge. Not surprisingly, McCullough has turned to the Panama Canal, describing its construction as “one of the supreme human achievements of all time” (p. 613). While the American attitude toward Europe and much of the rest of the world was often ambivalent during the period he describes, our Latin American policy was always governed by the Monroe Doctrine or by the United States' role as protector of Central American integrity. The corollary of these policies was the desire for acquisition of a waterway uniting the oceans which in the fullness of time erupted into veritable canal fever.

McCullough's treatment begins with the abortive effort of de Lesseps in the 1870s, proceeds to the growing American preoccupation, the debate over which isthmian route to follow, the dramatic lobbying activities on behalf of Panama, and, of course, the actual construction. American political, engineering, and medical perseverance succeeded in excavating the earth, controlling a river, solving the problems of locks and landslides, curing yellow fever, and creating a new nation. McCullough exalts three men above all—engineers John Stevens and George Goethals and the scientist who finally isolated that one pesty mos-

quito (out of the 2,500 species) which carried yellow fever, William Gorgas.

McCullough's view is all embracing; political and diplomatic events are of obvious concern but in many ways his description of the technological and social side of the construction is more enlightening. The account of the fierce discrepancy between white and black society, the negative conditions and exploitation endured by the laborers, and the good life enjoyed by only the chosen few is most revealing because it is not a very well-known story. Leisure outlets of the Zone dwellers, such as their reading habits, the dances and concerts they attended, and their club affiliations, are examined as part of McCullough's emphasis on social history. The sources used are commendable; manuscript and archival material from both sides of the Atlantic were mined and interviews with actual participants and old-time canal hands lend a refreshing dimension to his story.

Several minor caveats must be offered. Unfortunately McCullough's notes are at the end of the volume. They are not numbered but are identified by quotations and the pages where they appear, making them more difficult to use than conventional documentation. Despite the completeness of this volume, the author could have placed the story more concretely in the context of expanding American Caribbean concerns in the early twentieth century, since the canal certainly was both a cause and result of the policy.

McCullough makes clear that the building of the canal was a symbol of the promise of the twentieth century, the first real assertion of the United States as a global power, and an augury of things that would follow as the century unfolded. It is significant that this volume, so aptly describing this phenomenon, should appear at the same time as a new treaty which promises to adjust American-Panamanian relations by the turn of the next century. All in all, then, McCullough has fleshed in most colorfully a well-known and magnificent adventure. It is not surprising that this work has been a best-seller, and it is refreshing to review a book-of-the-month selection for this journal.

MILTON PLESUR
State University of New York,
Buffalo

HAROLD F. PETERSON. *Diplomat of the Americas: A Biography of William I. Buchanan (1852-1909)*. Albany: State University of New York Press. 1977. Pp. xvi, 458. \$40.00.

Harold F. Peterson's biography of William I. Buchanan will prove informative and useful primarily to historians of American foreign relations. Bu-

chanan was not a top echelon member of the United State diplomatic service. His contributions and abilities placed him on a secondary level, and yet it was precisely these secondary-level diplomats who performed much of the field work. We can only encourage biographical studies of other similar diplomats; even more desirable would be collective biographies of such officials.

One of the interesting themes which Peterson suggests, but unfortunately does not pursue adequately or analyze sufficiently, involves the inter-relationship between Buchanan's different careers from 1880 to 1909. In one career, Buchanan managed fairs and international expositions (Sioux City Corn Palaces, Chicago Colombian Exposition, Buffalo Pan-American Exposition), in a second he was a diplomat, and in a third served as overseas representative for several large American corporations, including New York Life Insurance and Westinghouse. Peterson observes that these three careers fitted nicely together for Buchanan and suggests they were mutually supportive, but he never sustains an analysis of their inter-relationship. Even without analysis, Peterson reveals interesting facets of the potential of such mutually supportive careers when he states that Buchanan had access to secret State Department codes and communications systems when working for private firms—a privilege extended to other American corporations (see Peter Collier and David Horowitz, *The Rockefellerers*).

The chief weaknesses in the biography seem to stem from the decision to focus narrowly upon Buchanan, following a style of historical biography which only seems to find relevance in documents to, by, and about its subject. This approach probably explains the author's tendency to cite regularly, in support of his own judgments about Buchanan, various formal, public, ceremonial laudatory remarks about Buchanan, and it contributes to his tendency to describe what Buchanan "must have" thought, done, or said (a few examples, pp. 137, 138, 183, 196, 198, and 199).

Another aspect of this biography which was good in conception but less satisfactory in execution is the author's consistent effort to create a description of the physical and social setting for each of Buchanan's career changes. Unfortunately, the selection of secondary works to recreate these historical periods often omitted important items, and thus important insights and descriptive elements are left out. For example, the background for Central America is derived solely from Dana Munro, overlooking fine studies by Mario Rodriguez, Ralph L. Woodward, and Thomas Karnes, among others.

In sum, we need biographies of men like Buchanan, and Peterson's study demonstrates mas-

tery of the facts of Buchanan's life while proposing interesting hypotheses regarding Buchanan's career, though the lack of analysis limits its ultimate utility. Nevertheless, it contributes to our understanding of how the foreign relations system of the United States functioned in the late nineteenth and early twentieth century.

THOMAS SCHOONOVER
Lafayette, Louisiana

JOHN MILTON COOPER, JR. *Walter Hines Page: The Southerner as American, 1855-1918*. (Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1977. Pp. xxx, 457. \$15.95.

Walter Hines Page had two careers: as writer, editor, and publisher in the United States, and as ambassador to Great Britain from 1913-18. John Milton Cooper decided that the most important and significant—though perhaps not the most interesting—part of Page's life was his work in the United States, and so he devotes approximately two-thirds of this biography to Page's activities before becoming American representative to the Court of St. James's. In this domain Cooper finds Page a man of considerable significance; he was, of course, a creative and successful editor—of *The Atlantic Monthly*, his own *World's Work*, and other periodicals, and cofounder of the publishing firm of Doubleday, Page and Company. Page's greatest contribution, so the author argues, came from his status as a southern expatriate living many years in the North, seeking to interpret and reform his former state (North Carolina) and former section. Page believed "that no separate 'Southern problem' existed . . . [that] the South's troubles were variations of general American conditions, worsened and complicated by economic and cultural backwardness, racial tensions, and misguided notions left over from the Lost Cause." It is possible, of course, to contend that these factors were large enough to make the South separate, and a different part of the United States. Cooper does not agree: "Page's insistence upon the essential unity of southern and American experience stakes his strongest claim to the rank of prophet" (p. xxiv-v).

In tracing Page's pre-ambassadorial career Cooper follows the path marked in the 1920s by Burton J. Hendrick's *The Life and Letters of Walter Hines Page*. This new book, however, is an authentic biography (which Hendrick's work was not), with much broader research, and more perceptive and objective interpretation. Page appears as a thoroughly fallible individual who smoked far too much (a major factor in his death in 1918), preferred to avoid disagreeable controversy, and usu-

ally acceded to his publishing partner, Frank N. Doubleday. He became a follower of Woodrow Wilson, even though in temperament and style, if not in program, he better resembled Theodore Roosevelt. Cooper takes frequent note of this fact but he does not explain it; in fact he does not develop well the early relationship with Wilson, and Page appears abruptly in 1911 as a promoter of Wilson's candidacy for the presidency. Otherwise the book offers a thorough, thoughtful account of Page's career in the United States.

The section on the ambassadorship is less original than other parts of the book. It covers familiar ground, includes many familiar—albeit colorful—quotations. Page was not a major cog in the wheel of Wilsonian diplomacy. Quickly identified as excessively pro-British, he was largely a forgotten and frustrated man during the period of the First World War. The treatment is, if anything, more negative than it would need to be. For example, the author becomes much more involved in describing Page's fall from grace with Wilson and Colonel Edward M. House than in establishing that he was influential in the first place. Cooper keeps attention focused directly on Page and avoids being drawn to related themes. The result is a volume with continuity and manageable length, but for examination of the issues, of Wilsonian diplomacy and British policy, one will have to look elsewhere.

These limitations for the most part are understandable. Page probably deserves a new biography and any such volume necessarily would include material that is not new. Cooper does add information on such topics as British intelligence operations (which included tapping the lines of the American Embassy), reasons for Page's death, and several aspects of his domestic career. He wisely carries the story beyond the ambassadorship, to publication of Page's fascinating letters, "a publishing sensation," in the 1920s. Page's moving accounts of wartime London, one might add, comprised a contribution perhaps as great as any of his efforts. Burton J. Hendrick prepared these volumes, writes the author, with Page's son Arthur "sometimes literally and always figuratively, looking over his shoulder" (p. 398). Understandably, the view was favorable, if not worshipful. Cooper's biography has helped place the life and career of Walter Page in clearer and more objective perspective.

ROSS GREGORY
Western Michigan University

ROBERT J. MADDOX. *The Unknown War with Russia: Wilson's Siberian Intervention*. San Rafael, Calif.: Presidio Press. 1977. Pp. ix, 156. \$9.95.

President Woodrow Wilson's controversial Russian policy has been the subject of many studies and conflicting interpretations. Robert J. Maddox, whose research in this area produced skillful and informative publications, has written a brief account of the major factors which influenced Wilson's response to the overthrow of the Provisional Government, the establishment of the Bolshevik regime, and the ensuing civil war. Maddox focuses on Wilson's decision to intervene militarily in Siberia; the complex, interlocking, and evolving issues and events at home and abroad provide the background.

Maddox joins the "soft revisionists" who contend that Wilson was motivated primarily by growing hostility to Bolshevism. Wilson's real and abiding aim was to overthrow the Bolsheviks before they consolidated their victory, threatened other areas, and endangered his plans for a stable and democratic postwar world; military intervention, hesitatingly approved by Wilson after intense allied pressure, and a host of other measures were designed to accomplish this. Maddox contends that Wilson masked his attitudes and goals by lofty principles and public moderation, while using pretexts and discretionary methods to advance his plan. Believing that he had to collaborate with the Allies, yet doubtful about events in Russia and the right course of action, Wilson became convinced that unabashed intervention would strengthen Bolshevism, aid objectionable Allied schemes, and increase domestic opposition, thus jeopardizing other objectives, such as the League of Nations. These apprehensions explain Wilson's vacillation and halfway commitment.

Overviews contain shortcomings. A bibliographical essay, for example, would have compensated for skimpy documentation and inadequate references to differences of opinion and interpretation. Maddox does not elaborate Wilson's plan for Russia or possible options. This holds particularly for social aspects of Russia's "reconstruction" and the relations of such a Russia with the United States. Siberia, of course, was only part of the Russian problem and Wilson had to address constantly issues he considered more important. Moreover, Wilson was a brake to more ambitious and deadly designs of his allies.

This is a useful addition to our knowledge. One hopes the book will be read by many laymen, some of whom may then wish to turn to other accounts for more detailed information about a little-known chapter in U.S. history.

N. H. GAWOREK
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Green Bay

FRANK E. VANDIVER. *Black Jack: The Life and Times of John J. Pershing*. In two volumes. College Station:

Texas A & M University Press. 1977. Pp. xxii, 594; 595-1178. \$35.00.

Frank E. Vandiver is a big man physically and mentally. He has the gift of a great storyteller and broad knowledge of military history that enable him to deal with the life of the outstanding American military leader of World War I. His two-volume work on General of the Armies John J. Pershing does much to catch the nature of the man and his contributions to the army and his country.

Biographies have a way of getting out of control. One sees this problem here when Vandiver's earlier leisurely approach and fullness of detail turn into a skeletonized version of the Chief of Staff era and the postwar days as space limitations forced a halt on the earlier type of treatment.

Except for an occasional over-sentimental touch and overuse of a onetime *Time* magazine practice of backing into sentences, the style is forceful and often eloquent. The format of the book is fine, the illustrations excellent, and the maps (sixteen for the two books) generous.

It is difficult to describe the elements that made the final leader. Descended from an Alsatian forebear who came to Pennsylvania in 1749, Pershing was born near Laclede, Missouri, in 1860. After a youth spent in farming, various odd jobs, teaching, and a short time at college, he entered West Point. There he set a record as a tough-minded cadet who ended as First Captain. Then, and again later, as an instructor at the Academy, he won the enmity of many cadets. Under different circumstances—in western posts guarding against hostile Indians and in the Philippines—the same qualities won respect, because his insistence on careful training and discipline paid off with victory.

Drawing partly on Don Smythe's earlier research, Vandiver disposes of the legend that Teddy Roosevelt, in a fit of pique because he could not reward Captain Pershing with promotion to major for his exploits in the Philippines, jumped him to brigadier general under regulations that gave him that power. Although Roosevelt mentioned such a possibility, the promotion did not come to pass without considerable pressure by Pershing's father-in-law, Senator Francis Warren of Wyoming, chairman of the Senate Military Affairs Committee. Indeed, Vandiver shows that Pershing, so often pictured as a hard-riding horseman in the field, was equally at home in the corridors of power in Washington and at the dances and receptions of the rich and influential.

In World War I, so much of Pershing's story is involved with training, arguments with Allied commanders over the proper use of American troops, and problems of supply that we do not get a full understanding of his tactical skill. The main element that comes through is his firmness in in-

sisting that American units would fight in Allied units only under American command.

Vandiver successfully shows us the human side of Pershing, so often hidden from those who held him in awe because of his toughness. Part of this comes from the diary of his wife and Pershing's correspondence during their courtship. The picture of domestic happiness comes through the narrative, and we are hit with double force by the savage blow that the general suffered when his wife and three daughters died in the fire at the Presidio in 1916.

Much of the story of World War I is that which has been presented by Pershing in his memoirs and by his associates. On the high-level disputes concerning the employment of American forces, Vandiver has introduced material not generally known. He has also reminded us of many associates of the general, now not well known, who made his victories possible: Liggett, Harbord, Bullard, Summerall, Dickman, Hugh Drum, Fox Conner, W. D. Connor, Frank McCoy. We know from other recent volumes of his dealings with Marshall, MacArthur, Patton, and March, but here we get another dimension. Vandiver has a knack for summing up personalities in a few lines so that we gain new perspectives on the general's dealings with contemporaries. Of especial value is the story of Pershing's relationships with President Wilson and Secretary of War Baker. The remarkable degree to which he was allowed a free hand in the field is made clear.

This long-awaited book by Frank Vandiver makes available the first authoritative and complete biography of Pershing.

FORREST C. POGUE
Smithsonian Institution

J. DAVID HOEVELER, JR. *The New Humanism: A Critique of Modern America, 1900-1940*. Charlottesville: University Press of Virginia, 1977. Pp. ix, 207. \$12.50.

The reaction against modern thought known as the New Humanism was germinated in Irving Babbitt's Harvard teachings early in the century, quickly and beautifully achieved its literary flowering in Paul Elmer More's *Shelburne Essays* (1904-10), and suffered a long drought of popular recognition. Rejecting opposing extremes of romantic individualism and pragmatic naturalism, Humanists posited a "higher self" in all human beings which could provide an "inner check" on natural impulses. Anxious not to rely on religious authority, Humanists asserted that the existence of the higher self was empirically demonstrable. Although David Hoeveler makes much of their empiricism, it seems, like tests of religious experience,

to have been purely subjective—the higher self could be felt within. Potential disciples like T. S. Eliot found Humanism an illogical and inadequate substitute for religion.

Sectarian narrowness usually prevented the Humanists from appreciating points of view similar in at least some respects to theirs—the critique of industrialism by the Agrarians, the dualistic interpretation of human nature by orthodox Protestants, and the opposition to literary naturalism of romantic liberals like Van Wyck Brooks and Randolph Bourne. Except for Stuart Sherman, the Humanists opposed reform movements as romantic assertions of individual good against social evil. Hoeveler attributes the movement's demise in the thirties to its political conservatism. The larger moral might be that those who would serve life cannot isolate themselves from it.

Essentially an academic movement composed of perhaps fifteen active professors, Humanists largely endorsed Babbitt's objections to spiritless teaching of the humanities by the "hustling scholar," to the elective system of Harvard's President Eliot, and to its corollary that "the wisdom of the ages is to be as naught compared with the inclination of a sophomore." Although the elective system was curbed, the Humanists fought a losing battle even in education, as is illustrated by Hoeveler's interesting account of Norman Foerster's career at the University of Iowa.

Somewhat sympathetic to the Humanists' disenchantment with modernism, Hoeveler is nevertheless critical of their ideas, which he ably, sometimes gracefully, explicates. But he is not so successful at establishing historical context. William Crary Brownell, an important precursor of Humanism, is totally absent from this account, and the attack on the movement by leftist writers in the early thirties is scarcely mentioned. This otherwise careful and useful book has the virtue of being the first full-length study of the New Humanism.

JAMES HOOPES
University of Miami

STUART I. ROCHESTER. *American Liberal Disillusionment in the Wake of World War I*. University Park: Pennsylvania State University Press, 1977. Pp. 172. \$13.50.

This brief book offers an intellectual history of liberal disillusionment from 1914 to 1940. The emphasis is upon the years 1914-20, with a concluding chapter sketching the primary pathways of the liberal mind between the wars. It is based on the published writings, supplemented by partial use of unpublished records (a casual set of notes hints at several missed archives) for thirty progressive or

liberal intellectuals. They are familiar names, with the *New Republic* men at the core, a group ranging from John Dewey and Lincoln Steffens to William Bullitt and David Starr Jordan, with Max Eastman and John Dos Passos angling across the stage.

Stuart Rochester seems to think these men and this problem "a neglected phenomenon" in historical writing. Perhaps this reviewer is jaded by his own modest efforts to address the same problem, but the field has been ably tilled by some of our best historians—among them White, Noble, Aaron, Goldman, May, Forcey, Lasch, Lawson, and Diggins. To make a new contribution would have required an effort Rochester did not mount. If a "definitive" synthesis is now conceivable, it would surely rest upon a more exhaustive search of primary records, including oral histories, and would reach out to more than his list of less than three dozen men. Having chosen not to do that, Rochester has produced a book which falls considerably short of being a major work.

This seems to me especially unfortunate, for the author shows signs of talent. Although basically presenting the hoary interpretation that the progressive/liberal intellectual was an innocent who "fell prey to foolish notions and grandiose illusions," "overexpected and miscalculated," (p. 90), and "fulminated rather than reflecting on the true nature or depth of the problem," (p. 81), Rochester develops a personal view of the importance of World War I with some skill. He accepts much of the continuity view of Henry May, yet rather persuasively reassesses the view of the war as watershed. He has a flair for writing, which firm editing might preserve from its excesses of overripe phrasing and snappy clichés. This is another book which should have taken longer to ponder, research, and write. A reviewer in a national journal is the last in a professional chain in the academic and publishing worlds, and if each would resist the pressures exerted upon (and by) young scholars to rush into print the reviewer would not have to offer such public and belated advice. I am ready to concede only one reasonable rebuttal, that the historical world has some use for a level-headed, compressed study of a subject that has occupied some of our best scholarly talent, even if it now seems not quite so burning a cluster of problems. Perhaps this is so. If it is, then this is that book, and it would be handier for those who need it if it were in paperback.

OTIS L. GRAHAM, JR.
University of California,
Santa Barbara

MEYER WEINBERG. *A Chance to Learn: The History of Race and Education in the United States*. New York:

Cambridge University Press. 1977. Pp. viii, 471. Cloth \$27.50, paper \$6.95.

This is the most comprehensive study of a subject neglected in most histories in general, and in histories of American education in particular. There are separate sections on the history of children's education in four minority groups—black, Mexican-American, Indian-American, and Puerto Rican (there is nothing on the education of Asian-American children).

The history of the education of minorities in this country is an underdeveloped field of research; so Meyer Weinberg's contribution is welcome. There is much new data here, and there is a lengthy bibliography which is invaluable. But *A Chance to Learn* must be used with caution. This is a history of race and education in America, not "the" history, and a very special sort of history at that. Not just because it is inevitably sketchy—139 pages on black children, 37 on Mexican-American, 29 on Puerto Rican, and 51 on Indian-American—but in view of the following problem as well. The history of the education of minority groups is a subject which is frequently polemicized by those who investigate it. Educational historians have tended to interpret the subject in accordance with their own political and social bias. The dominant perspective until roughly the past decade has been laudatory if not utopian—The Little Red School House was the highest realization of the democratic ideal the world has ever seen. Newer interpretations have tended toward the opposite—a radically negative attitude. Both interpretations are polemic in intent, the one intent on proving the success of the American public school, the other intent on proving its failure. The latter seems to be in the ascendancy today.

Weinberg writes in this tradition: "Public school authorities . . . all but deserted minority children. . . . Professional organizations of teachers and administrators collaborated actively in maintaining the racial order. . . . White educators profited from the enforced absence of black and other minority competitors for jobs. Planned deprivation became a norm of educational practice. . . . A contempt for minorities is expressed even in the most authoritative histories of education" (pp. 1, 3). In another but highly relevant context, C. Vann Woodward has elaborated the problems of white liberal historians writing on black and minority history ("Clio with Soul," *JAH*, 56[1969]: 5-20). Will historians be able to absorb and control their outraged moral passions without losing balance and betraying principle? Or will the historian's moral engagement compromise the integrity of his craft? *A Chance to Learn* provides an exemplary footnote to Woodward's implicit warning. Wein-

berg is so predictable that even the new evidence he marshals seems familiar, not to say stale.

The truth is too complicated to be explained by an a priori position of hostility toward schools and schoolmen. Each minority group must be studied separately, within the context of its own traditions and interests and with due consideration of the historical context. When each group is seen on its own terms several points emerge: First, the groups are of course not monolithic. Second, the way a particular minority group was treated differed, sometimes dramatically, from one historical period to another, and from one region of the country to another. Third, even within the same time period, minority groups often encountered different educational policies in different cities, states, regions. Finally, not all education of minorities took place in public schools, and the decision to respect the right of minority groups to maintain private schools was itself public policy. In short, what is needed is more nuance and more discernment.

SOL COHEN
University of California,
Los Angeles

HASIA R. DINER. *In the Almost Promised Land: American Jews and Blacks, 1915-1935*. (Contributions in American History, number 59.) Westport, Conn.: Greenwood Press. 1977. Pp. xvii, 271. \$17.50.

As Hasia Diner correctly points out, "when American historians have written about race they have talked in two categories only—black and white. . . . This kind of analysis," she says, "ignores the subtleties and complexities of American ethnic identity." Many scholars in recent years have noted the need for studies of the relationship between blacks and white ethnic groups, but few have appeared. Even more surprising, despite countless journalistic analyses of contemporary Jewish-black relations, this is only the second full-length historical study of the subject. Unlike the first, *Bittersweet Encounter*, by Robert Weisbord and Arthur Stein, which focused on black anti-Semitism, Diner's study is primarily concerned with Jewish attitudes toward blacks. Specifically, her book deals with the views of Jewish leaders on black issues in one twenty-year period—beginning with the lynching of Leo Frank in 1915 and ending with the Harlem riots of 1935. Diner is not concerned with black history *per se* nor with Jewish-black relationships on the mass level. Instead, she confines herself to a survey of articles on black issues in selected Yiddish and English-language Jewish publications and a study of Jewish participation in civil rights activities, black philanthropy, and the organization of black labor.

Within the limits she has set for herself, Diner makes a significant contribution. She demonstrates persuasively that during these years the leaders of the Jewish community took a consistent position in support of black civil rights and black economic and political advancement. The black struggle was viewed sympathetically by Jewish leaders of every political persuasion, from the Socialist Abraham Cahan to the conservative Julius Rosenwald, and was clearly of greater interest to the Jewish community than any other non-Jewish issue. Diner argues that this special Jewish-black alliance flowed out of the needs of the Jewish community at a time when the status of Jews in American life was uncertain. "Jewish leaders played with the issues of racism and black status as a way of working out certain problems and tensions of American life" (p. xv). Still reluctant to confront anti-Semitism directly, Jewish leaders could express their frustrations with discrimination in American society by criticizing the treatment of blacks. Moreover, Diner argues, by championing black causes, Jews were able to demonstrate the depth of their commitment to the rhetoric of American democracy. And, at the same time, by adhering to a higher moral standard than other white Americans, they could fulfill their role as a chosen people.

Diner's thesis is only partially convincing. Certainly, the ambivalent status of Jews in American life in the early twentieth century contributed to the singular support which Jewish leaders gave to black causes. But why did other white ethnic groups, with similar problems in gaining acceptance, fail to use the black struggle as a forum to express their own frustrations? Why did other white ethnics frequently emerge as vocal opponents of black advancement? The answer may lie less in the statements of community leaders than in the nature of day-to-day intergroup contact. Because of their occupational patterns and their remarkable upward mobility, Jews were less likely than other immigrant groups to be in direct economic competition with blacks. In fact, the economic relationship between blacks and Jews, despite occasional tension between blacks and Jewish merchants and landlords, was in this period largely symbiotic. By concentrating exclusively on Jewish leadership, Diner overlooks this dimension. Moreover, Diner is less than persuasive when she maintains that support of black causes was a means by which Jews could express their Americanism. Most white Americans in this period were unflinchingly racist, and pro-black attitudes, despite the American creed, were out of the American mainstream. Support of black causes may have brought Jewish leaders into alliance with a few New England neoabolitionists, but it

increased their distance from most white Americans.

Diner's book helps explain why a special relationship between Jews and blacks developed within the context of a particular historical period and why that relationship ultimately ended. Although her perspective is limited and her arguments not always convincing, she has nevertheless expanded our understanding of an important aspect of American race relations.

ALLAN H. SPEAR
University of Minnesota,
Twin Cities

LESTER C. LAMON. *Black Tennesseans, 1900-1930*. (Twentieth-Century America Series.) Knoxville: University of Tennessee Press. 1977. Pp. xi, 320. \$13.50.

This book is a valuable addition to the growing literature on the status of Southern blacks during the early years of the twentieth century. It traces the evolution in white racism during the first decade and a half of the century, analyzes the mixed blessings of the World War I boom, and reveals the stagnant, in some ways even declining, position of Tennessee blacks in the 1920s. But rather than break new ground, Lester C. Lamon's findings basically follow current trends in black historiography. He argues that the state's blacks "were not simply acted upon by white society; they created, destroyed, and responded to national and local stimuli [especially progressivism] in their own right" (p. x). Yet for them, as for earlier generations of blacks, "the overriding feature of life . . . was the *separate* community—separated from the recognized white mainstream partly by force and partly by choice" (p. vii). And most importantly, a Washingtonian-inspired accommodation to white discrimination remained the basic black response. Despite increasing, though still occasional, examples of protest that included several streetcar boycotts, the formation of NAACP branches, the political assertiveness of Memphis' Robert Church, Jr., and a student strike at Nashville's Fisk University, "most black Tennesseans came to accept another postponement (but not cancellation) of the 'dream'" (p. 297).

Tennessee is an especially good choice for this kind of study, for as the author suggests, its three distinct regions provide a cross-section of the white South's treatment of blacks. With the partial exception of political participation, conditions for blacks worsened as one moved westward from the comparatively liberal atmosphere in Knoxville, Chattanooga, and Nashville to the more oppressive racial environment in and around Memphis.

Similarly, the absence of a uniform black response mirrored the divisions among Southern blacks in general. In dealing with the variations in black attitudes and tactics, Lamon makes good use of black sources such as interviews and the previously overlooked *Nashville Globe*.

There are, however, troubling problems in conceptualization. Despite its title, this book is actually about Tennessee race relations. There is no consideration of such fundamental aspects of the black experience as religion, class structure, cultural life, or family organization. Instead, and indicative of the author's primary interest in racial ideology, there are a series of often overlapping chapters that chronicle the interaction between prominent blacks and whites in areas such as business, politics, public accommodations, and especially, education. Nor does the author satisfactorily explain his decision to concentrate on conditions in the state's four major urban centers to the neglect of the majority of Tennessee blacks who lived in rural areas. Lamon's choice of dates is also puzzling. Of necessity, periodization is often arbitrary, but it would seem that the years between 1900 and 1930 form a more natural unit for the study of Northern rather than Southern blacks, particularly in Tennessee. Lamon fails to confront the evidence of increased political, social, and economic discrimination in Tennessee after 1890, thus ignoring the critical antecedents for the actions of both blacks and whites after 1900. And given Lamon's own findings, the years 1920, 1940, or even 1954 would all constitute more likely terminal points than 1930. That year did mark the first time that a majority of Tennessee blacks were urban (50.3%), but the author does not cite this as a reason for his cut-off date. Indeed, what is missing is any attempt to explain the choice of either the opening or closing date. A more complete concluding chapter could have dealt with this problem, as well as making clearer the ways in which Tennessee blacks were better or worse off in 1930 than in 1900.

HOWARD N. RABINOWITZ
University of New Mexico

WAYNE FLYNT. *Cracker Messiah: Governor Sidney J. Catts of Florida*. (Southern Biography Series.) Baton Rouge: Louisiana State University Press. 1977. Pp. xiv, 359. \$20.00.

In 1916 Sidney J. Catts, a virtual novice in politics, was elected governor of Florida; he kept the state in turmoil during his term and periodically for the next decade. Like the earlier Governor Napoleon Bonaparte Broward, Catts initially had no prominent political connections, but, unlike Broward, he

was a demagogue whose victories often came by appealing to his followers' ignorance and lower instincts.

Catts' personal history reveals much of the tragedy of the New South. He had received a law degree and inherited a substantial plantation in Dallas County, Alabama, yet he was the product of a frontier environment whose intellectual and social decline permitted an individual to combine formal learning with bigotry and arrogance. After his religious conversion in 1886, Catts became a fundamentalist minister and served Alabama churches until financial difficulties with his plantation led him to accept a "call" to a church at Defuniak Springs, Florida in 1911. After three years he resigned to become a traveling insurance salesman, a position which enabled him to assess the popular will and develop techniques which increased his appeal when he ran for governor. Following a campaign in which he championed anti-Catholicism and prohibition, Catts was denied a primary victory by court decisions in contested precincts. Not to be daunted, he ran as a Prohibition Party candidate and won in the general election.

In his stormy years in office, Catts' violent temper, imprudent actions, and lack of political finesse produced disappointing results. He crassly removed many officeholders, tried unsuccessfully to obtain reapportionment, and, although his alliance with labor was genuine, failed to secure enactment of important labor legislation. At first he advocated tax equalization but later aided in destruction of the tax commission as an economy measure. He did obtain establishment of a state highway system, increased funds for education, passage of the prohibition amendment, and the end of state convict leasing, but was unsuccessful in getting approval of the suffrage amendment. Catts ran for the U.S. Senate in 1920, again emphasizing anti-Catholicism, but he was decisively defeated by the moderately conservative Duncan U. Fletcher. In 1924 and 1928, he also ran unsuccessfully for governor.

Wayne Flynt emphasizes the importance of Catts' religion in his actions but acknowledges that it "locked him into a mental frame of reference which saw only 'good guys' and 'bad guys' without the 'proximate solutions' that make American politics work" (p. 341). Moreover, from the standpoint of his self-proclaimed convictions, he sold out on fundamental moral issues, swapping votes for appointments and, in his 1928 race, accepting local options on racing and, probably, gambler support.

The absence of a large body of Catts' papers deprived Flynt of the intimate insights needed for an in-depth analysis of Catts, but his careful use of

oral history and the papers of Catts' associates enabled him to present Catts in a meaningful, sympathetic, but critical fashion. His careful study is more than a biography since it also portrays the history of Florida in an important era of transition.

HUGH C. BAILEY
Francis Marion College

CHARLES R. HEARN. *The American Dream in the Great Depression*. (Contributions in American Studies, number 28.) Westport, Conn.: Greenwood Press. 1977. Pp. x, 222. \$15.95.

As its title suggests, this book is a study of what happened to the American ideology of success during the Depression, using both popular and "serious" fiction as well as self-help books of the period as source material. In essence, the author argues that in the 1920s such criticism as there was of the American dream was in the novels of "serious" writers like Dreiser, Fitzgerald, and Lewis, while in popular magazine stories and self-help books, "success was conceived almost invariably in terms of business success, and the businessman was revered beyond belief." In the 1930s, however, Charles Hearn finds that while serious writers become still more critical of the American Dream, even popular sources show various degrees of doubt and disillusion, if not outright apostasy. He analyzes this material into several types: 1.) attempts to reaffirm the ideology of success which show that "a large number of people were not yet ready to give up their image of America as a Horatio Alger paradise." Hearn suggests, however, that this traditional type of success literature differed from the past in that "the dream of success and happiness became an end in itself, a fantasy which freed the individual from reality instead of infusing him with the energy and ambition needed to cope with it"; 2.) expressions by writers like O'Neill, Farrell, Steinbeck, and Caldwell of the theme of the American dream as a fantasy or delusion in the lives of their characters; 3.) the proliferation in both popular and serious forms of protagonists who are failures by the traditional definition of success—little men, losers, and outsiders. Here, for example, Hearn discusses the significance of the highly popular gangster film in relation to the ideal of success; 4.) the popularity of new kinds of guidebooks such as Lin Yutang's *The Importance of Living* and Walter Pitkin's *Life Begins at Forty*, which stressed leisure instead of work, and self-fulfillment instead of rising in the world; and 5.) direct attacks on the gospel of success as in Nathanael West's savage and bitter satires.

There are no major discoveries in this book since it is no great surprise to discover that the Great

Panic and Depression engendered doubts about the American dream in the minds of many Americans. Because the author's emphasis is on the interpretation of texts, he is not able to give us much information about which parts of the public were affected by these various strands of criticism, something one would like to know more about. Nonetheless, as a broad and responsible survey of the American dream as represented in both popular and serious literature of the 1930s, this is a solid and useful study, which should be consulted both by historians and literary historians of the period.

JOHN G. CAWELTI
University of Chicago

ROBERT CRAIG WEST. *Banking Reform and the Federal Reserve, 1863-1923*. Ithaca: Cornell University Press. 1977. Pp. 243. \$14.50.

In exploring the bank reform movement of the late nineteenth century and its impact on shaping the Federal Reserve Act, Robert Craig West amply illustrates that bank "reform" is an evolutionary process responding through the political process to changes in the economic and intellectual environment. After a sketchy survey of U.S. banking between 1836 and 1908, West examines various reform proposals discussed between 1863 and 1908, divided into those dealing with the medium of exchange problem and those related to organization. After discussing the views of Paul Warburg and Victor Morawetz and their often overlooked contribution to the origins of the Federal Reserve, West reviews the attempts to formulate a banking reform bill. The Aldrich Bill stressed a central bank, asset-backed currency, and private control while the Glass Bill provided for more government involvement in central banking but a less centralized organization. The congressional debate centered on how much and what to control.

West distinguishes between a strong form of the real bills doctrine which stressed the self-regulating nature of credit if banks invested only in real bills, and a weak form which rejected the notion that such a system would be self-regulating. Both sides agreed on the basics of a discount mechanism based on commercial bills but disagreed primarily over the organization of the new system. According to West, the impact of the real bills doctrine on those drafting the act was considerable, but its impact on early system operation has generally been overemphasized. By the time the act was passed, the two major theoretical underpinnings, the real bills doctrine and the international gold standard, were no longer an important part of the financial environment. The Federal Reserve, therefore, began operations in a world different

from that envisioned by its drafters and only slowly began to find its identity and purpose in the post-1920 period. West concludes that the early Federal Reserve did not embrace the strong form of the real bills doctrine, but his assertion that it very early relied on open market operations as well as the discount rate does not seem supported by the evidence. The final chapter covers the changing and evolving organizational structure of the system. Debate centered on the number and location of banks, the locus of power, and policy and control. The Federal Reserve System as it evolved came to resemble more closely the National Reserve Association envisioned in the Aldrich Bill.

The book's major contribution is to re-emphasize the political manner in which bank legislation is formulated, discussed, and finally passed in the United States. The extensive discussion of various reform proposals and the ensuing congressional debates does not add substantially to our understanding of the issues. While it is useful to discuss suggested changes in the light of the existing intellectual environment, it would have helped to explore the economics of these more straightforwardly.

RICHARD H. KEEHN
University of Wisconsin—
Parkside

DOROTHY GANFIELD FOWLER. *Unmailable: Congress and the Post Office*. Athens: University of Georgia Press. 1977. Pp. x, 266. \$14.50.

Does a citizen of the United States have a right to use the United States mails or is that a privilege conferred by Congress? Does freedom of the press include, in addition to the right to publish, the obligation of the Post Office to circulate the published material? In her book, *Unmailable*, Dorothy Ganfield Fowler seeks to answer these and other questions relating to the postal service and to show how and why the answers changed over the years. Overall, she is much more successful explaining how than why.

Early in her book the author traces the development of the Post Office, the violations of the privacy of letters mailed during the first years of the Republic, and the familiar story of incendiary antislavery propaganda, which was never declared unmailable but simply kept from circulation through the South by southern postmasters. During the Civil War, Fowler notes, letters from southerners to northerners and northern newspapers of doubtful loyalty were kept from the mails. But this was done by the postmaster general at the instigation first of the Department of State, then of the

Department of War. Only after the war did Congress itself say what was unmailable.

By the 1870s Congress' list of proscribed mail matter included material relating to lotteries and "every obscene, lewd, or lascivious article or thing designed . . . for the prevention of conception or procuring abortion . . ." (p. 63). Gradually the list grew. At the end of World War I, obscene letters, even if sealed, mail to perpetrate frauds, "filthy" literature, and treasonable and anarchistic literature were all unmailable.

Congress resisted pressure to add to the list, but in order to help the states protect the health and morals of their citizens, it did declare unmailable such things as pest-ridden plants and firearms that could be concealed on one's person.

For years the Supreme Court upheld the constitutionality of these laws and the postmaster generals' fraud orders as well, ruling that citizens have no constitutional right to use the mails and that excluding certain publications from the mails abridged only the right to mail, not to publish. But after World War II Supreme Court decisions restricting the postmaster general's use of the fraud orders and making community standards and literary quality tests for obscenity virtually unraveled the skein of laws governing unmailable matter.

Fowler has written a timely book. It is carefully constructed from primary sources and is a useful summary of the laws and judicial interpretations of those laws relating to unmailable matter. Unfortunately, it is less useful in enlarging our understanding of the forces in American life that shaped, then rejected, those same laws.

WAYNE E. FULLER
The University of Texas,
El Paso

KLAUS-GEORG WEY. *Das U.S. Department of Commerce and Labor und Interessenverbände, 1903-1913*. (European University Papers. Series 3, number 69.) Frankfurt: Peter Lang. 1976. Pp. vi, 237. DM 36.

As compared to the advanced industrial nations of Europe, government intervention in economic relations is a fairly new tradition in America. The historical roots of this tradition are the subject of the study under review, a dissertation presented at the University of Cologne which deals with the relationship existing between the U.S. Department of Commerce and Labor and American pressure groups at the beginning of this century. The founding of the department itself in 1903 resulted from interaction between the government and business interests. Klaus-Georg Wey shows that among its many duties the department continuously emphasized the promotion of American ex-

ports. Refusing to serve the special interests of particular branches of industry, it sought to bring about an organization which represented the whole of the American business community. Only in 1912 did it definitely succeed, and a Chamber of Commerce of the United States was formed. A further indication of its limited influence was given by the modest results it achieved in its attempts to regulate business activities through its Bureau of Corporations and its Steamboat Inspection Service. One reason for this weakness originated in a policy conflict; on the one hand, the department realized the need for some concentration in a modernized economy, and on the other hand, it wanted to preserve the free market system.

Its relationship to labor (through its Bureau of Labor and Statistics) was similarly ambivalent. Wey explains why this was not only the department's but also the unions' fault. Trying to be "impartial" it provided information on the employment situation, but declined to advocate labor-friendly measures. Commissioner C. P. Neill changed this stand somewhat; he had an investigation of working conditions in the steel industry conducted. But this revised policy failed to dispel the reservations of most labor organizations vis-à-vis the new institution. They continued to resent federal intervention in social matters (including immigrant labor problems) as an attempt to undermine their political position. The author concludes that in spite of these difficulties the work done by the department had not been in vain. Important new experience was gained in the field of government-business relations, which bore fruit when World War I required large-scale government intervention in the economy.

Wey has made the best use of his complex source material, taken mostly from the department files. The only objection one might raise is that his summary remains too general in showing the connections between the early stages of government intervention, which form his subject, and the further development of government-business and government-labor relations in the United States. But this is a minor drawback which should not prevent a translation of this informative study treating a somewhat neglected subject.

KLAUS SCHWABE
University of Frankfurt

HANS J. KLEINSTEUBER. *Staatsintervention und Verkehrspolitik in des USA: Die Interstate Commerce Commission. Ein Beitrag zur politischen Ökonomie der Vereinigten Staaten von Amerika*. (American Studies, number 46.) Stuttgart: J. B. Metzlersche. 1977. Pp. ix, 255.

Hans J. Kleinsteuber has tried to fill a gap in the German literature on U.S. economic regulation by

writing a book on the "largest, oldest, and most widely investigated regulatory commission," the Interstate Commerce Commission. The results of the study, based mainly on government documents and books and articles, are summarized in chapters on the economic and political situation of the late nineteenth century, the legislative history of the Interstate Commerce Act, the structure and procedure of the ICC, its position in the American political system, and the economic consequences of ICC regulation.

In dealing with such a broad subject as the ICC it is obviously difficult to grasp all important aspects. A choice has to be made, and it can be expected that the choice will be either to follow a standard text-book approach or, better, to present and prove the author's own hypothesis. Unfortunately, Kleinsteuber does neither, and in fact explicitly denies any intention of putting forward a particular interpretation. The material dealt with is somewhat unbalanced, both in extent and quality.

The author uses thirty-nine pages to characterize the history of the railroad business and the political discussions that preceded the Interstate Commerce Act. He presents long quotations from original sources in order to show the nature of the railroad problem, but fails to discuss clearly enough the major interpretations on the subject, and to make the idea and content of the ICA explicit. He next summarizes the legislative changes and supplements of the act up to the 1970s, but does not give adequate information about underlying and related economic and political developments.

The best parts of the book concentrate on the activities and problems of the ICC today. The German reader gets the information necessary to understand a type of administrative agency very different from European ones. It is pointed out that the agency's ability to mobilize sufficient support from Congress and well-organized interest groups is the main reason for the maintenance of the ICC in its existing form, in spite of general criticism as to the anticonsumer orientation of the agency and the economic consequences of this sort of regulation.

Finally, one must point out that there are too many mistakes in orthography and style in the book.

KLAUS-GEORG WEY
Meerbusch, Germany

MELVYN DUBOFSKY and WARREN VAN TINE. *John L. Lewis*. New York: Quadrangle. 1977. Pp. xvii, 619. \$20.00.

From the hitherto untapped materials in the archives of the United Mine Workers of America, the

oral histories of the eminent men and women who crossed his path, and an impressive number of private and public manuscript collections, Melvyn Dubofsky and Warren Van Tine have constructed a monumental biography of a cynical but impressive John L. Lewis. Dubofsky and Van Tine develop six themes. They offer a case study in the myth of the self-made man and an examination of a life which encompassed the evolution of union leadership in America from a calling to a career—an evolution which also embodied the trade-union shift from protest and conflict to collaboration and accommodation with capital.

Lewis, they show, also moved from advocating the use of federal power to strengthen trade unions in the 1920s and 1930s to criticizing the power of the state in the 1940s. He also epitomized what the authors call the "ends and means" problem of managing large organizations such as the United Mine Workers of America. W. A. "Tony" Boyle, former UMW president who was convicted in 1975 of having ordered the murders of Joseph A. "Jock" Yablonski, his wife, and his daughter, was, after all, a Lewis lieutenant who cut his teeth on labor violence with Lewis' tacit approval.

Lewis' career does illuminate their themes and reveals the "tensions inherent in voluntaristic institutions between democratic ideals and the inevitable centralization of power in modern bureaucracies." The story of his life not only makes for dramatic narration but provides "an examination of the emergence of a professional, rational, bureaucratic labor movement" (pp. xv-xvi).

The authors convincingly revise earlier interpretations—particularly the legends. Gone, therefore, are the fables: the union leader descended from a line of Welsh coal miners; wife Myrta Bell's tutoring the rough-hewn John in the classics and teaching him diction; Franklin D. Roosevelt's rejection of Lewis for the vice-presidential slot in 1940 as the cause of the feud between the two men. Gone, too, is Lewis' masterminding of Section 7a as the means to "fertilize the egg of the CIO," and his "plot" to create a rival federation by means of the Committee for Industrial Organization. In place of these tales, Dubofsky and Van Tine offer a picture of a less visionary, more ordinary—even bourgeois—but still opportunistic and unscrupulous Lewis who responded to ever-changing events as other mortals do.

Lewis, the authors conclude, was an "enigma, wrapped in a riddle cloaked in mystery" (p. 292). Eschewing systematic psychohistorical interpretation, they focus on his public life. But they also provide rich psychobiographical material: Lewis' obsession with death after his brother George died in 1931; Lewis' sublimation of his aggressive energy into his role as labor leader—often thereby reducing all public issues to conspir-

acies and personalities; his clear distinction between home and career; his total isolation during times of crisis; his "eagerness to inflict pain on rivals" (p. 73); and the close relationship with "Daddy's girl" Kathryn but estrangement from son John Jr. Lewis, they remind readers, humiliated AFL President William Green in the mid-1930s by charging that Green was sitting "with the women, under an awning on the hilltop," instead of returning to "his father's house," to "the union that suckled him" (pp. 225-38).

In their concentration on events as seen through Lewis' eyes, the authors have been overly generous about the economic results achieved by the labor leader's stabilization proposals for the coal industry during the Depression. The Lewis-sponsored Guffey-Vinson Act of 1937 did not, as the authors suggest, provide "the federal rationalization of the soft-coal industry that Lewis for more than a decade had sought fruitlessly" (p. 375). Rather it failed to establish effective minimum-price levels for the industry until 1942—when the nation at war needed not minimum but maximum prices—and became a bureaucratic embarrassment to the New Deal. Otherwise, this is a splendid book. In fact, Dubofsky and Van Tine's volume will become the standard work on Lewis.

JAMES P. JOHNSON
Brooklyn College,
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ROBERT H. ZIEGER. *Madison's Battery Workers, 1934-1952: A History of Federal Labor Union 19587*. (ILR Paperback, number 16.) Ithaca: New York State School of Industrial and Labor Relations, Cornell University. 1977. Pp. xiv, 126. \$4.50.

Federal labor unions are among the more unusual and intriguing organizing bodies utilized by the American labor movement. Chartered directly by the American Federation of Labor, these locals were never intended as permanent unions but rather were conceived as short-term organizing units whose membership would eventually be divided among appropriate craft unions. As the AFL's major instrument for organizing mass-production workers, federal labor unions assumed added significance from the organizing opportunities created by enactment of the National Industrial Recovery Act and the National Labor Relations Act during the 1930s. Indeed, such large and influential international unions as the United Auto Workers, the Rubber Workers, and the Electrical, Radio and Machine Workers sprang directly from the federal union structure.

Robert H. Zieger's study of Federal Labor Union 19587 in Madison, Wisconsin is a welcome first effort to chronicle the history of one of these

unions. Composed of Ray-O-Vac dry-cell battery production workers, the Madison local received a charter from the AFL in May 1934 and remained a federal local until affiliating with the United Auto Workers in 1963. Unfortunately, Local 19587 can hardly be described as a typical federal union. Its location, unusual longevity, ideological conservatism, and subdued economic militancy were uncharacteristic of most such unions during the 1930s and 40s. Perhaps least typical of all, a complete set of the records of Federal Local 19587 was preserved and deposited at the State Historical Society of Wisconsin.

The above qualifications notwithstanding, Zieger has produced a useful study filled with good sense and sound judgment. His critical but sympathetic portrait of the union rank and file avoids the romanticism that has crept into labor history lately, and his analysis of the relationship between union members, both active and passive, and union leaders contributes significantly to an understanding of local union affairs. Surprisingly in such a short monograph, Zieger succeeds in "capturing something of the human reality of one group of workers" and in "illuminating broader patterns of labor activism" (p. vi).

GARY M FINK
Georgia State University

THOMAS R. BROOKS. *Communications Workers of America: The Story of a Union*. New York: Mason/Charter. 1977. Pp. xiii, 257. \$12.95.

The history of a union of 750,000 members, more than half of them women, is necessarily of great importance today when interest in organized labor and the woman worker is at a high point. Unfortunately, Thomas Brooks' study of the *Communications Workers of America* adds little either to the general question of women workers and trade unionism or to the specific development of this particular union, whose members range from switchboard operators to engineers to cable splicers. One would expect that the study of an industry in which women occupied so important a place would devote adequate space to a discussion of the general problems facing women workers in the American labor movement. Yet all we get is the statement: "Unionization of women workers was rare at the time" (p. 10). This for the period on the eve of World War I. The reasons for this situation and its relationship to the attitudes and policies of male labor leaders of the trade unions, especially those of the American Federation of Labor, are not even mentioned.

Later, Brooks raises some interesting questions with respect to women in the Communications Workers of America but fails to provide meaning-

ful answers. He writes, "Women have always played a greater role within CWA than in most unions with a substantial female membership" (p. 238). But then he points to the "recent absence of women at the very top level of leadership" and explains it in part by the introduction of automation which "reduced the proportions of operators, heretofore a woman's job." The relevance of this explanation escapes this reviewer. We are told on the same page that "today, in sharp contrast to ten or fifteen years ago, CWA membership reflects the national averages for minority participation in the work force, ranging twenty percent black and six-to-seven percent Latino." Why the situation was so vastly different "ten or fifteen years ago" is not even discussed. This is of some significance, one would suppose, in a book dedicated to Joseph A. Beirne, key organizer and strategist of the union, author of such works as *New Horizons for American Labor* and *Challenge to Labor*. Incidentally, while Brooks refers to Beirne's role in the founding and operation in Latin America of the American Institute for Free Labor Development, organized under the aegis of the AFL-CIO (p. 240), he does not even bother to tackle the troublesome issue of the purported relationship of the CIA to the AIFLD.

Back in 1952 Jack Barbash published *Unions and Telephones: The Story of The Communications Workers of America*. There is not much in Brooks' 1977 work which was not dealt with in 1952. Nevertheless, the period since 1952 occupies less than 50 out of 245 pages. A much more useful and effective work would have emerged if the author had summarized the period already covered by Barbash and developed the recent period in much greater detail. Finally, the bibliography is less than adequate. There are no reference notes, but we are assured: "Most of my sources are indicated in the text." An accompanying guide to the location of these sources would be useful.

PHILIP S. FONER
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Pennsylvania

DICK MEISTER and ANNE LOFTIS. *A Long Time Coming: The Struggle to Unionize America's Farm Workers*. New York: Macmillan Publishing Company. 1977. Pp. xi, 241. \$14.95.

The triumph of Cesar Chavez's United Farm Workers of America (UFW) over California's grape growers at Delano in 1969-70 precipitated a journalistic and scholarly reappraisal of agricultural unionism in the United States. Not since John Steinbeck focused attention on California's agrarian labor conflicts of the 1930s and Paul Taylor and Carey MacWilliams explored the plight of

Mexican and Mexican-American farmworkers has so much been written about class conflict in the fields. In the past seven years more than a dozen scholarly and popular books have been published which describe either the struggle of farmworkers to unionize or the life of Cesar Chavez. Dick Meister and Anne Loftis' book is the most recent and comprehensive of such studies.

Writing largely in simple journalistic prose and eschewing analysis for description, Meister and Loftis explore the shifting ethnic-racial composition of California's farm workers; the history of that state's pre-World War II agricultural unionism; the rise and decline of the Southern Tenant Farmers' Union; and the triumph of unionism among Hawaii's multiethnic plantation workers. For these topics, the coauthors rely on existing secondary literature and several unpublished dissertations. Yet these explorations serve merely as an introduction to the book's central story.

With the appearance of Chavez as union organizer, the authors, especially Meister, who, as a reporter for the San Francisco *Chronicle*, covered many of the events herein described, come to the heart of their tale. Their prose now sparkles and their knowledge of the subject widens and deepens. We see clearly how the pre-Chavez efforts at organizing agricultural workers failed, owing to the AFL-CIO's half-hearted commitment; the inability of organizers to understand the culture of the workers; cultural splits between Chicano and Filipino laborers; and, of course, the relentless and brutal antiunionism of most growers. Then we learn how Chavez synthesized the workers' Roman Catholic beliefs with their Mexican *campesino* protest traditions to build a union which the farmworkers considered their own and for which they sacrificed. Winning support from the Roman Catholic hierarchy, Robert Kennedy, Walter Reuther, even a hitherto reluctant George Meany, and millions of consumers who boycotted California grapes and lettuce, Chavez's union brought organization and some security to thousands of toilers in our "factories in the fields." Meister and Loftis even bring their story to the eve of publication. Their final chapters describe the UFW's struggle against the alliance between the Teamsters' Union and the growers; the UFW's victories in the union representation elections mandated by California's 1975 Agricultural Labor Relations Act; and the defeat of the farm workers' initiative to broaden union rights under the 1975 statute. Despite the UFW's defeat in the 1976 election, the authors close on an optimistic note, agreeing with Chavez, "*Si se puede*. It can be done."

Its lack of scholarly documentation and analysis notwithstanding, the book provides a painless introduction to the history of agricultural unionism

in the United States. It should be useful to historians unfamiliar with the topic and to students eager for more knowledge about farm workers. But its price is rather high.

MELVYN DUBOFSKY
State University of New York,
Binghamton

MARK A. STOLER. *The Politics of the Second Front: American Military Planning and Diplomacy in Coalition Warfare, 1941-1943*. (Contributions in Military History, number 12.) Westport, Conn.: Greenwood Press. 1977. Pp. xiii, 244. \$16.95.

Acrimony, deception, and broken promises littered the path to the Normandy invasion. To placate each other and the folks back home the three Allied leaders agreed to an early second front that each, including Stalin, knew would not be mounted. To the last, Churchill sought to scrap or delay Normandy in hopes of pursuing his Mediterranean strategy. Meanwhile, whenever it seemed they would not get their way, the Americans childishly threatened to take their ships and guns to the Pacific, while the Russians hinted ominously at a separate peace with Germany. The games went on at the highest levels, where Churchill and Stalin needed each other cruelly.

Through it all, each nation carefully pursued its national interest—not least the American Joint Chiefs, accused by an earlier generation of critics of being political morons oblivious to the long-range implications of strategy. American army leaders, Mark A. Stoler asserts, carefully calculated the benefits of a second front—how it would strengthen the army's position vis-à-vis the navy, appease a public impatient with the prospect of a long war of attrition, and place American soldiers where American interests were greatest, in Western Europe. More than that, a second front would satisfy Russian demands for help in battling Germany while also making the West strong enough on the continent to counter Soviet domination there. Thus it left all options open—the perfect instrument of policy for a leadership which could neither trust the Russians nor do without them, which feared Moscow but still hoped to cooperate with it after the war. Still, Roosevelt himself vacillated, and British and American negotiators argued bitterly (so bitterly that once, “when an experiment with pistols was conducted during one of the closed sessions, the guards outside concluded that the military leaders had finally begun to shoot one another” [p. 114]).

According to Stoler, only Stalin's renewed insistence on the invasion at Tehran guaranteed its future. Stoler perhaps gives Stalin too much credit.

Equally important in tipping the balance from British to American strategy was the diminishing power of Britain—“a poor donkey,” as Churchill saw it, caught between “the great Russian bear” and “the great American buffalo” (p. 150). Such matters of nuance aside, however, Stoler's basic point is incontestable: if initially outfoxed by the British, American generals soon played political hardball with the best; if initially confused, American military strategy soon expressed national interest effectively.

This is no longer startling news to historians, who have given military affairs increasing attention in recent years. Forrest Pogue has shown how sensitive Marshall was to political matters, and a growing body of monographic literature has detailed the political interests and world-view of America's wartime leadership and its role in the onset of the Cold War. Furthermore, the undergraduate reader may find this book difficult, since it does not develop the broader context of the course of the war, Russian-American relations, or the historical development of American military leadership. But the specialist will find this a useful monograph—well documented, meticulously detailed, and tightly if somewhat colorlessly written. It helps confirm recent reinterpretations of American military policy, reconstructs an important political debate, and increases our understanding of the origins of the Cold War.

MICHAEL SHERRY
Northwestern University

ERNEST F. FISHER, JR. *The Mediterranean Theater of Operations: Cassino to the Alps*. (United States Army in World War II.) Washington, D.C.: Center of Military History, United States Army. 1977. Pp. xxiii, 384. \$17.00.

Within weeks, the spotlight, which had never shone all that brightly on Italy, was going to shift to Northwest Europe. Thereafter, the Mediterranean would not only fall into the shadows as far as public attention was concerned, but would become as well a secondary theater in terms of troops, supplies, and (U.S.) strategy. On May 11, 1944 the U.S. Fifth Army, under Lieutenant General Mark W. Clark, and the British Eighth Army, under Lieutenant General Sir Oliver Leese, began a drive on both sides of Monte Cassino, seventy-five miles southeast of Rome, to break the German Gustav Line. These are the circumstances, the time, and the place with which Ernest R. Fisher, Jr. begins the fourth and concluding volume of the U.S. Army's Mediterranean Theater subseries.

The climax comes early in the volume, as it did in the campaign, with the fall of Rome on June 4, a

scant two days before D-Day in Normandy. It was a moment of glory, just about the last. Cassino, the Rapido River, Anzio, and Rome had seemed to be what the war in Italy was all about. Everything that came later would, as Fisher puts it, "appear to be a postscript, a vast holding operation." The next episode of high drama would not occur until the very end when the German surrender brought onto the scene the SS General Karl Wolff and Allen Dulles, the U.S. intelligence chief in Switzerland. But much would happen in between on a three-hundred-mile advance through mountains and over rivers in nearly eleven months of fighting.

Before they passed north of Rome the armies were already on notice that they would soon have to give up part of their strength to the projected invasion of southern France. The pursuit was short-lived and came to a stop on the Trasimeno Line in the third week of June. From there on the Fifth and Eighth Armies had to fight their way north with diminishing forces; in July to Leghorn, Arezzo, and Anconia; in August across the Arno River and to the Gothic Line; and into the Winter Line south of Bologna in October. Then, through the winter and into the next spring, they faced having to work their way out of the mountains, across the Po River, and up to the Alps.

Fisher follows the tactical fortunes of the troops on both sides with all of the precision readers have come to expect in the U.S. Army in World War II series. He also addresses forthrightly the problems of coalition warfare, and they were many in Italy.

One problem was the intermingling of personal and national ambitions with tactics, such as Clark's conduct of Fifth Army operations in the race to Rome. Others were the differences of strategic opinion, mostly (but not always, as Fisher shows) on national lines; the arguments over the relative priorities of Italy and southern France; and the convolutions of the Mediterranean strategy in general. All of these and more have for the historian become wrapped up in one big question: was the march up the Italian boot worth the effort, particularly after D-Day? Fisher indicates that it was not, even before D-Day. The Allies, he says, could have contained as many Germans at less cost to themselves had they stopped in the Naples-Foggia area, north of Rome, or somewhere in the northern Apennines.

The dedication in all U.S. Army in World War II volumes reads, "... to Those Who Served." In *Northwest Africa* (Howe), *Sicily and the Surrender of Italy* (Garland and Smyth), *Salerno to Cassino* (Blumenson), and *Cassino to the Alps* those who served in the Mediterranean Theater have received their due as far as history can give them that.

EARL F. ZIEMKE
University of Georgia

DOMINIC J. CAPECI, JR. *The Harlem Riot of 1943*. Philadelphia: Temple University Press. 1977. Pp. xiii, 262. \$15.00.

In this first comprehensive study of the Harlem Riot of 1943, Dominic J. Capeci, Jr. has accomplished more than a detailed description of a long-neglected racial incident in American history. He has also provided readers with a deeper understanding of race relations in the United States during the years of the Great Depression, the New Deal, and World War II. As a result, Capeci's focus is broader than Harlem or New York City, and his appraisal is more comprehensive than that of most other studies of a single racial incident in American history. He describes in vivid fashion the effects of the Great Depression upon blacks in Harlem and the efforts of both the masses and their leaders to respond to this crisis. By contrasting the approaches of Mayor Fiorello H. La Guardia and Adam Clayton Powell, Jr. to the problems of depression, Capeci not only points out the complexity of ghetto life in the 1930s but also offers new insights into the personalities of two controversial politicians.

War-time conditions, more than economic depression, however, have traditionally given rise to racial resentments which frequently spill over into violence, and World War II was no exception. While the vast majority of white Americans were at least enjoying the fruits of war-time prosperity, black Americans continued to face discrimination in jobs, housing, and education on the home front and segregation in the armed services. The situation in Harlem was more serious than either President Franklin D. Roosevelt or Mayor La Guardia realized. Roosevelt was too preoccupied with war-time considerations to pay much attention to racial matters, and La Guardia, not unreasonably, believed his gradualistic program of civil rights and his own sensitivity to the depressed lot of poor blacks in Harlem would prevent racial violence from occurring in New York City. After Harlem failed to explode in wake of the Detroit Riot of June 1943, the leaders of New York City were so reassured that they ignored black discontent over high prices and rents and the failure of city government to curb police brutality. Despite black protest, La Guardia allowed the U.S. Navy to rent Hunter College, permitted the police department to close the famous Savoy Ballroom in Harlem, and refused to block plans to construct a segregated public housing unit in Manhattan's lower east side. In each case, blacks felt betrayed by these decisions.

The Harlem Riot of 1943 was touched off on August 1, when a white policeman allegedly mistreated a black woman and a black soldier in

uniform at a Harlem hotel where prostitution was known to exist. In the twelve hours of ensuing violence, six persons died, several hundred others were injured, and approximately two million dollars' worth of property was destroyed. Although the swift and effective actions of Mayor La Guardia kept loss of life and property to a minimum, New Yorkers, both black and white, tried to conceal the racial aspects of the riot by blaming it upon hoodlums. As Capeci correctly points out, these efforts spared the city's immediate reputation, but in the long run the price was high, since public officials failed to take appropriate action to lessen the tensions which had produced the violence in the first place.

The Harlem Riot of 1943 is a first-rate study of a significant racial incident which contributes to a better understanding of race relations in the United States. Hopefully this book will stimulate Capeci, or some other competent historian, to produce an equally good study of the Detroit Riot of 1943.

ROBERT V. HAYNES
University of Houston

ALAN M. OSUR. *Blacks in the Army Air Forces during World War II: The Problem of Race Relations*. Washington, D.C.: Office of Air Force History. 1977. Pp. 227. \$2.40.

A flurry of recent studies on Afro-Americans and the Second World War amply demonstrates both the historical profession's renewed interest in the effects of war on society and its continued search for a "watershed" in the struggle for black freedom. Most of the literature echoes Richard Dalfume's assertion that the changes in race relations during the war period, their causes and consequences, constitute the "forgotten years" of the Negro revolution. Alan Osur's monograph is no exception.

Blacks in the Army Air Forces during World War II examines how and why this service made headway toward the equitable treatment of Afro-Americans. It is by no means, however, an exercise in exoneration or an attempt to vindicate what the Air Force did. Osur makes clear the limited actual gains made by blacks and the efforts of the white Army Air Force leadership to forestall and minimize the racial reforms promulgated by the War Department after 1943. Nevertheless, Osur emphasizes the discontinuities which laid the groundwork for the eventual desegregation of the armed services. His central theme is the evolutionary modification of Army Air Force racial policy and practices. First, the Air Corps abandoned its policy of excluding blacks. Then the service retreated

from its plan to keep blacks restricted to mainland service battalions. Gradually it provided technical training for blacks, permitted them to fly twin-engine aircraft, sent them overseas in increasing numbers, and even ordered them into combat operations. At the same time, the service's demand for total segregation, and its overtly discriminatory practices, diminished somewhat as it inched along toward compliance with a policy of equal treatment for all personnel. These changes, according to Osur, resulted from the protests and pressures of the black community. An organized and militant black protest movement forced the War Department to broaden opportunities for Afro-Americans while, concomitantly, arousing black servicemen to act in ways which led to yet greater alterations in Army Air Force practices. Black Air Force personnel fought against segregation and discrimination by deliberate lethargy and low morale, by violence against individual whites, by full-scale demonstrations, and by riot. These manifestations of dissatisfaction ultimately caused the War Department, in pursuit of its stated aim of military efficiency and effectiveness, to call for an end to segregation and unequal treatment.

Osur, in sum, follows the lead of Dalfume, Lee, Nichols, Paszek, and Stillman in stressing the decisive first steps toward integration that occurred during World War II. Although competently written and objectively presented, his study is neither unique in assessment nor innovative in approach. His narrowly focused and researched monograph adds little to what is already known. It will be of interest mainly to specialists seeking a few additional bits of information.

HARVARD SITKOFF
University of New Hampshire

JESSIE A. GARRETT and RONALD C. LARSON, editors. *Camp and Community: Manzanar and the Owens Valley*. Fullerton: Oral History Program, California State University. 1977. Pp. xvi, 233. \$7.95.

This book contributes a new dimension to the study of the internment of West Coast American Japanese during World War II. A "relocation center" is viewed through the eyes of Caucasian neighbors in the small communities outside the barbed-wire-enclosed compound. Twenty long-time residents of the Owens Valley in eastern California describe their reactions to Manzanar, known locally as the "Jap camp," where approximately ten thousand Issei and Nisei were detained thirty years ago.

Most of the interviews were conducted in 1973 by Arthur A. Hansen, director, and David J. Bertagnoli of the Japanese American Oral History

Project at California State University, Fullerton, adding to the growing number of oral history transcripts on this subject. CSUF earlier published *Voices Long Silent: An Oral Inquiry into the Japanese American Evacuation*, edited by Hansen and Betty E. Mitson.

This volume, edited by two other scholars in the CSUF program, is generously illustrated with photographs of Manzanar during World War II and today, reprints of contemporary newspaper stories, and pictures of evacuee artifacts, some of which are now displayed at the Eastern California Museum in Independence.

The illustrations provide the historical background for the interviews. The Owens Valley people were asked to comment on the wording of a state landmark at the camp site, dedicated in April 1973, which refers to Manzanar as the first of ten "concentration camps" and condemns the "hysteria, racism and economic exploitation" involved in the removal of 110,000 persons of Japanese ancestry from the West Coast. For those interviewed, this question posed the problem of reconciling the reactions of 1942 with current attitudes.

Many spoke of initial fearfulness and hostility toward the evacuees in the context of anti-Japanese wartime propaganda. Lack of previous contact with Asians led to racial stereotyping. Though the construction of the camp and the presence of the military guards stimulated the lagging economy of Inyo County, the outnumbered Caucasians wanted the prisoners to stay inside their fence.

We learn that their fears were allayed somewhat by a government-instigated public relations campaign in the local newspapers and by the appointment of two respected local men as chief and assistant project directors of Manzanar. After a year there was no panic when elderly Issei left the compound to fish or hunt for rocks in the mountains.

Most of those interviewed reveal themselves as indifferent to the real issues of the evacuation, unable to see the American Japanese as individuals. Only two women seemed aware of the traumas described in evacuee accounts of life at Manzanar. An interview with one of them, a local historian, is an articulate commentary on the social distance between camp and community.

ANNE LOFTIS
Menlo Park, California

JUDITH M. GANSBERG. *Stalag: U.S.A.: The Remarkable Story of German POWs in America*. New York: Thomas Y. Crowell. 1977. Pp. ix, 233. \$9.95.

Judith Gansberg has written an interesting book on a little-known subject: the incarceration and re-education of German prisoners of war in the United States during World War II. Many of the

more than 370,000 POWs in this country underwent a program designed to turn them against Nazism and create a body of democratically inclined citizens to assist in the administration of postwar Germany and help guide that nation in a democratic direction. The Prisoner of War Special Projects Division, a group of sympathetic army men, scholars, and educators, was created in 1944 and kept secret because such a program contravenes the Geneva Convention on prisoners of war. The planning, implementation, and effects of this project provide the meat of this well-written study.

Gansberg is properly restrained in evaluating the program's success. Evidence indicating pro-democratic attitudes among prisoners who underwent re-education is balanced by recollections of some of the former "students" who thought less of the program than their captors. The relatively high number of the program's graduates willing to play some political role in postwar Germany must be measured against the fact that the POWs from the United States generally exhibited more liberal attitudes than Germans who remained in their homeland during the war. The most convincing argument for democracy among most prisoners was practical rather than ideological: the visible prosperity of America. Democracy, they believed, meant good living. Among the confirmed Nazis in the camps, fewer than three percent were converted to the virtues of our political system.

Gansberg offers a number of interesting observations on the treatment of the POWs. First, because the American military men who ran the camps were naïve about German politics, Nazi prisoners gained control in many of the camps and terrorized and occasionally murdered anti-Nazis among the inmates. It took a journalistic exposé and the direct intervention of Eleanor Roosevelt to bring about some reform. Second, although Germany was our enemy and Russia our ally, the pro-Nazi and pro-Communist prisoners of war were considered equally dangerous, and neither type was selected for participation in the major re-education program. In fact, U.S. Army officers and civilians working in the project were often harassed by red-baiting Congressmen or denied military clearance if their politics were too liberal, a story all too familiar to anyone who has worked in the history of World War II civilian-military agencies.

The book might have benefited from more of the German viewpoint, which was inevitably more skeptical than that of the program's American sponsors. Otherwise this is a sound study of the treatment of what were surely the best cared for prisoners of war the world has ever seen.

SYDNEY WEINBERG
Ramapo College of New Jersey

HOWARD ROFFMAN. *Understanding the Cold War: A Study of the Cold War in the Interwar Period*. Cranbury, N.J.: Fairleigh Dickinson University Press. 1977. Pp. 198. \$9.50.

This book was written as an undergraduate honors thesis in history at the University of Pennsylvania. Subtitled "A Study of the Cold War in the Interwar Period," its major thesis is that the period from 1937 to 1939 was crucial, if not determinative, in the development of Cold War antagonisms after the Second World War. Howard Roffman contends that Great Britain and France chose to appease Hitler rather than work with Stalin because the latter course might have led to Soviet predominance in Eastern Europe. "Britain's leaders faced the decision of which dictator they would have to come to terms with to avoid war, and they chose wrong" (p. 28). They made this mistake because they misunderstood both Hitler and Stalin; in the case of the latter, the leaders of the West were so blinded by their paranoia toward Communism that they could not make an unbiased decision that might have halted Hitler. Stalin, it is argued, would have been more than willing to have entered a military alliance but was rebuffed by the West; the events of 1937-39 set his attitudes against the West, with disastrous results after Hitler's defeat in 1945. The author believes that Franklin Roosevelt at that time "would have been willing to accept Soviet hegemony in Eastern Europe, provided that Stalin's excesses were curbed to the degree that they did not hopelessly limit Roosevelt's political maneuverability at home" (p. 187). But his successor, Truman, followed the example set by Great Britain and France from 1937 to 1939, and this only increased the firmness of Stalin's control over Eastern Europe. The lesson is clear. If only the West had negotiated the Russians into the Western camp in 1937, and if Roosevelt had not died, the Cold War antagonisms that developed after the Second World War would have not occurred.

Written almost exclusively from Western secondary and published primary sources, and thus lacking manuscript sources from either the Western powers or the Soviet Union, the work gives the Soviet Union the benefit of any doubts. While the Western powers are portrayed as weak, stupid, and indecisive, the Soviet Union is represented as strong, intelligent, and determined. Almost all Russian moves are logical, while almost all Western ones are unwise. A lack of documentation on Soviet motivation and goals makes such a conclusion possible. While the subtitle specifies the interwar period, the years from 1921 to 1937 are dealt with in a cursory manner. The author's concluding pages on Roosevelt and Truman seem

strangely out of place in a book whose content and footnote citations deal preponderantly with British attitudes and developments; very little foundation was laid for such a conclusion. And, finally, Roffman simply does not make a convincing case for his interpretation. The book is written with too much hindsight and too little investigation into such factors as British public opinion, internal political conditions, and the British decision-making process. The author's contentions may well be true, but he has not proven his case.

THOMAS BUCKLEY
University of Tulsa

Foreign Relations of the United States, 1949. Volume 5, *Eastern Europe; the Soviet Union*. (Department of State Publication 8852.) Washington, D.C.: Government Printing Office. 1976. Pp. x, 1011. \$11.25.

Judging from the material in this volume of *Foreign Relations*, the United States and Soviet Union by 1949 had settled into what could be described as the routine diplomatic business of the Cold War. The papers chronicle no immediate crises straining American relations with the Soviet Union and Eastern Europe.

The publication begins with a section entitled "Multilateral Relations." It examines east-west economic contact, trade, human rights issues, and other related topics. Sections then follow on each East European nation, including a large segment devoted to the Soviet Union.

The relationship of the Soviet Union to its satellites is one topic of primary concern in these diverse documents. Many American policy-makers believed that the Soviet Union had lost ground in Europe. Inspired by this, they concentrated on how to further weaken the Soviet bloc. They examined potential means to this end, ranging from using Voice of America broadcasts to exploiting the Vatican's opposition to Communism (based on the fact that Eastern Europe had a sizable Catholic population). But they constantly returned to the conviction that America's most potent weapon was its economic preponderance. Exemplary of this frame of mind was a report to the President dated December 8, 1949, which at one point declared that "it is probably in the economic realm that we can most concretely make our influence felt" (p. 53). Numerous documents reflect America's preoccupation with wielding this economic strength.

The Soviet-Yugoslavian split was seen as one notable instance of the decline of Soviet influence. It encouraged American policy-makers while presenting new problems to be faced. Since Tito was a

Communist, American economic assistance for his nation was approached cautiously. Economic incentives might further alienate Tito from the Soviets; but, among other reservations voiced, George Kennan warned that the United States might confuse the public by expressing a dislike for Communism in any form while openly supporting Tito for practical reasons (p. 960). Furthermore, although the possibility admittedly was remote, the United States had to determine to what extent it would support Yugoslavia in the event it was invaded by the Soviet Union or another Eastern European nation.

The section on the Soviet Union details constant diplomatic sparring. The State Department had to respond to Soviet claims of diplomatic immunity for Valentin Gubichev, who, along with American Judith Coplan, was charged with espionage. Efforts to settle Russia's wartime lend-lease debts continued to complicate relations. The Soviet detonation of an atomic weapon aroused concern, though it surprisingly received only limited treatment in this volume. Taken in isolation, these and other exchanges seem rather trivial. Taken as a whole, they portray growing Cold War distrust and rivalry.

The papers are well edited and clearly organized. The editors often omit lengthy analyses from some documents, but editorial notes provide good summaries of the omissions. Numerous footnotes help tie together vast amounts of material in a coherent fashion. Cold War historians will find a wealth of details to substantiate many already established views, but this volume provides no strikingly new insights into policy decisions.

T. MICHAEL RUDDY
St. Louis University

Foreign Relations of the United States, 1949. Volume 6, The Near East, South Asia, and Africa. (Department of State Publication, number 8885.) Washington, D.C.: Government Printing Office. 1977. Pp. xi, 1,852. \$16.50.

Here, in the State Department's longest volume to date—1,852 pages—is a massive collection of cables, letters, and memoranda on American relations with the Middle East, the Indian subcontinent, and Africa during 1949. Consistent with the relative importance assigned by American policymakers, less than 150 pages are devoted to all of South Asia and Africa, whereas the section on Israel constitutes more than half the volume.

Clearly demonstrated are Washington's desire for an even-handed approach to Israel and the difficulty of attaining this goal. The United States hoped the UN would find a way to guarantee

Israel's existence on terms acceptable to the Arabs. Although Dr. Ralph Bunche managed to mediate a truce in May 1949, the Palestine Conciliation Commission (PCC) failed to produce a political settlement. Washington then turned its hopes to a UN Economic Survey Mission under Gordon Clapp of TVA. But the Mission's call for New Deal-type intraregional development and public works projects ignored mutual suspicion and hostility in the area.

Palestinian refugees posed a particularly difficult dilemma. Commendable as the American contribution of sixteen million dollars for relief may have been, there was no long-term solution. The Arab nations could afford neither to maintain the Palestinians nor integrate them into largely subsistence economies. Israel proved so recalcitrant about "repatriation" that President Harry S. Truman angrily wrote his representative on the PCC, "I am rather disgusted with the manner in which the Jews are approaching the refugee problem" (p. 957). While everyone waited, the number of refugees grew from seven hundred thousand to nine hundred fifty thousand.

Although the United States supported a strong and independent Israel as a bulwark against Soviet designs on the Middle East, Israel earned American condemnation early in 1949 for her incursions into Egypt against el-Arish and the Negev, later for her "constant, defiant and threatening" (p. 873) attitude in negotiation, and finally for beginning to relocate her government to Jerusalem.

American military aid, designed to strengthen existing regimes against Soviet attack or subversion, was requested by nearly all countries and granted to many. When the Shahanshah of Iran visited Washington in November, for example, the State Department supported what the American ambassador in Tehran called the Shah's "obsession" (p. 583) with medium tanks, less to strengthen the Iranian Army than to bolster His Majesty's "morale" (p. 590), the most important guarantee of Iran's continuing role in containment of the Soviet Union. Military aid to Greece prevented an insurgent victory, but it was the termination of Yugoslav support for the insurgents that created the condition for an end to the civil war. Military aid alone did not defeat the guerrillas so long as they enjoyed external support.

Historians will welcome the evidence on the development of American policies, but security restrictions continue to block access to some documents. References to unprinted National Security Council paper 18/4, "US Policy Toward the Conflict Between the USSR and Yugoslavia," and to unprinted Central Intelligence Agency Intelligence Memorandum 180 on the Palestinian refugees, only tantalize the reader. In addition, as

the NSC gathered institutional direction over policy, based increasingly on reports from the CIA, the State Department's role was reduced. Access to other agencies' papers thus becomes more and more essential to competent scholarly reconstruction and evaluation of recent American foreign policy.

RICHARD PFAU
Dickinson College

Foreign Relations of the United States, 1950. Volume 1, National Security Affairs; Foreign Economic Policy. (Department of State Publication, number 8887.) Washington, D.C.: Government Printing Office. 1977. Pp. xi, 945. \$11.00.

The major areas covered by the documents selected for publication in this volume of the Foreign Relations series are U.S. policy with respect to nuclear weapons, national security generally, the reconstruction of Western Europe, and the effort to build a stable Western political economy excluding the Soviet Union, East Europe, and Communist China. Indeed, approximately two-thirds of the volume is taken up with the national security question, and most of the remaining documents are concerned with the effort to build a stable political economy. After a careful reading of the documents, I have concluded that the proportions are about right. In the one area where one might have raised questions about exclusions, i.e., the papers related to the Gordon Gray report (the dollar gap), the editors have carefully listed the titles and even box locations in the National Archives of all the significant unprinted papers. This is an example of the kind of careful editing which continues to make *Foreign Relations* the key preliminary guide to the basic sources for the study of the foreign policy of the United States. In this respect I can think of no substantive improvements on the choices of papers printed in this volume.

The very inclusiveness of the volume suggests several significant interpretive possibilities. For example, the documents tend to lead to four not always mutually reconcilable interpretive trends with reference to U.S. policy on control of production of nuclear weapons: that U.S. proposals for control of nuclear weapons were not meant to be accepted by the Soviets, because, as Dean Rusk put it on January 6, 1950, U.S. proposals gave "no prospect of producing international control" (p. 9); that American leaders had a genuine fear that Soviet aggression was highly probable by 1953-54; that it was only U.S. nuclear supremacy which prevented the Soviets from using their conventional military supremacy from overrunning Western Europe; but, as Secretary of State Dean Ache-

son assured several militant Senators in January of 1950, that "Russia does not want war," and "all our energies must be directed toward supplying the necessary funds to do the many things necessary to win the cold war" (p. 141).

If we are to accept the priority of hierarchy in policy-making then we might well hypothesize that the Cold War was not, in the secretary of state's view, a kind of prelude to war or actual war with Russia. But how are we to get over the knotty fact that the documents also reveal a genuine fear of Soviet attack? These documents suggest a number of possibilities about how to resolve that paradox. If Russia did not want war, yet might attack, such an attack might be expected to arise out of developments which were planned to take place between 1950 and 1954. One possibility was that the Soviets might attempt to achieve nuclear parity by stockpiling atomic or developing hydrogen bombs, thus gaining supremacy in Europe. But the U.S. could anticipate that by stockpiling and developing a hydrogen bomb itself. Another possibility was that the U.S. would succeed in rebuilding West Europe economically and creating rough equality with the Soviets in Europe in conventional armaments under the aegis of NATO. This would give the United States and its allies military supremacy. Such supremacy would have the result of changing the balance of power in Europe against the Soviet Union. In the past wars had sprung out of such immediate causes. That is to say, American leaders might well have expected that Soviet leaders might have gone to war to protect the existing balance of power favorable to the Soviets, which American power was altering.

Although nothing can yet be said conclusively, this last—and expected—Soviet attack in reaction to a change in the European balance of power caused by the United States seems the most likely way to reconcile the mutual contradictions in the American positions. NSC 68 then becomes a shield of United States nuclear strength behind which the European economy could be rebuilt and a basis laid for a West European system of self-defense, when combined with the economic development and commercial treaty policies outlined in the documents composing the last third of the volume.

CARL PARRINI
Northern Illinois University

JOHN BARTLOW MARTIN. *Adlai Stevenson and the World: The Life of Adlai E. Stevenson*. Garden City, N.Y.: Doubleday and Company. 1977. Pp. 946. \$15.00.

In *Adlai Stevenson of Illinois*, John Bartlow Martin traced the life of Stevenson through his Illinois

career and unsuccessful bid for the presidency in 1952 (*AHR*, April 1977). He concluded with the observation that "the best years of Adlai Stevenson's leadership lay ahead." In the present work, Martin completes the story to Stevenson's death in 1965.

During the Eisenhower years, Stevenson maintained a leadership role in the same manner he had achieved it before the 1952 convention: by not openly seeking it. Under his sponsorship, a number of the nation's leading Democratic liberals (Thomas Finletter, John Kenneth Galbraith, Chester Bowles, Averell Harriman, Arthur Schlesinger, jr., and others) met periodically to develop position papers. The "Finletter Group" thus laid the groundwork for Stevenson's 1956 campaign. More importantly, according to the author, these position papers became the basis of the New Frontier and the Great Society when the Democrats returned to power in the 1960s.

Stevenson traveled abroad during these years, and foreign policy continued to dominate his interests. In 1956 he successfully sought his party's nomination, and lost to Eisenhower that fall by an even greater margin than in 1952. Four years later, when John Kennedy narrowly defeated Richard Nixon for the presidency, most observers assumed Stevenson would become secretary of state, a position he ardently desired. Kennedy decided that he was too controversial for this post, however, and appointed him instead as ambassador to the United Nations. Although Stevenson thus became a part of the New Frontier administration, personal relations were short of the ideal. He considered John Kennedy a wealthy upstart in politics, and the young president, in turn, believed the "Governor" was indecisive, even in their most confidential meetings. Their relationship was further strained when Stevenson discovered that he was dispensing false information to the world about the Bay of Pigs fiasco. His colleagues at the UN assured him of their sympathy, for their superiors had also sometimes lied to them!

Despite this and other difficulties, Stevenson was at his best in representing his country at the UN, especially with his "until hell freezes over" speech in the Cuban missile crisis. He successfully prepared the world for Kennedy's foreign policy and, during his tenure, the American people supported the United Nations more warmly than they ever had before or since. Following Kennedy's assassination, Stevenson expected to play a larger role in formulating foreign policy, but again he was disappointed, as Dean Rusk became more influential with Lyndon Johnson than he had been with Kennedy. Stevenson's last year at the UN was unpleasant, for he had to defend America's position in the increasingly unpopular war in Vietnam.

During his service at the United Nations, Stevenson journeyed throughout the world, accepted numerous honorary degrees, and socialized with wealthy, beautiful women and movie stars. Although he had been repeatedly warned by his physicians, he continued to eat too much, to gain too much weight, and to work too hard. He died suddenly of a heart attack on a London sidewalk in the summer of 1965.

This volume is even more minutely detailed than the first one—at times almost boringly so. The style is very readable, although the author has a penchant for long quotes and incomplete sentences. Again, as in the first volume, the Stevenson papers and oral history interviews were well utilized, and Stevenson's personality continues to emerge vividly. The most disappointing omission, to this reviewer, is a general lack of conclusions. The author does not attempt to evaluate Stevenson's place in history, except to assert that his 1956 campaign was his greatest contribution, providing the foundation for the programs of the 1960s. Criticisms aside, these two volumes will probably constitute the definitive biography of Stevenson. What is still needed is a succinct, scholarly, interpretive study. John Bartlow Martin's encyclopedic account will provide a rich source for that future biographer.

R. ALTON LEE
University of South Dakota

WALTER JOHNSON, editor. *The Papers of Adlai E. Stevenson*. Volume 7, *Continuing Education and the Unfinished Business of American Society, 1957-1961*. Boston: Little, Brown and Company. 1977. Pp. xv, 655. \$22.50.

Although academic interest in Adlai Stevenson declined during the attack on liberalism in the late 1960s, a modest revival has been sparked by the editing of Stevenson's papers by an old partisan, historian Walter Johnson. In this seventh, penultimate volume, Johnson includes materials from the period 1957-61, from the second defeat to Stevenson's assumption of duties as ambassador to the United Nations. The essential theme of the selections is Stevenson as prophet—as a transcending figure escaping the worst excesses of Cold War rhetoric and molding the issues and idioms later employed by Kennedy-Johnson liberals.

While obviously inconclusive, the *Papers* do suggest that Stevenson was not quite the Cold Warrior recent critics have suggested. Something of an apologist for a dying colonialist system, he urged the Eisenhower administration to seek a stable, non-Communist Third World through economic, rather than military, assistance. Stevenson contin-

ued to prod the administration to take a more flexible attitude in the nuclear test-ban negotiations and to accept the principle of military parity with the Russians as a first step toward broader disarmament. By 1960 he had clearly staked out an advanced position among the presidential contenders on the test-ban and disarmament questions and had even received an oblique offer of aid from Premier Khrushchev, an offer Stevenson emphatically rejected.

Unquestionably Stevenson had also emerged as the liberal Democrats' most articulate critic of the affluent and apathetic society at home. Using an idiom of national peril and moral urgency, he attacked the Republicans for permitting, even encouraging, economic stagnation and a sterile sense of complacency. While condemning President Eisenhower for a lack of leadership on the school desegregation question, however, Stevenson himself was a gradualist with a disdain for "intolerant northern liberals" (p. 61) and a distinctly paternalistic attitude toward southern liberals.

Despite public denials, Stevenson retained presidential aspirations after the 1956 defeat, and he sought to shape party issues through the newly created Democratic Advisory Council. His apparent breadth of vision, his world travels, his engaging personal style, and his unmatched literary talents permitted him to retain his devoted following within the liberal establishment, which vainly tried to draft him for a third nomination in 1960. A disappointed Stevenson campaigned extensively for the Democratic ticket and hoped to become Kennedy's secretary of state. Political memories and personal incompatibility apparently prompted Kennedy to offer only the UN ambassadorship.

Although widely admired, Stevenson remained a lonely figure—concerned about his sons and his weight and increasingly dependent for emotional support upon a number of wealthy and influential matrons. By 1960 his speeches and correspondence reveal an increasing stridency toward the Republicans, a loathing of Richard Nixon, and an aversion for the "ruthless" (p. 520) tactics of the "arrogant" (p. 521) and ungrateful young Kennedy. This volume of *Stevenson Papers* will become a valuable source for future research on postwar liberalism, the test-ban debate, and hopefully for a much needed scholarly biography of Stevenson himself.

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EDWIN BICKFORD HOOPER *et al.* *The United States Navy and the Vietnam Conflict*. Volume 1, *The Setting of the Stage to 1959*. Washington, D.C.: Naval History

Division, Department of the Navy. 1976. Pp. xi, 419. \$9.00.

This volume is the first in a planned multi-volume official history of the United States Navy in Vietnam. On the assumption that events in Vietnam should be viewed against the backdrop of American involvement in global Cold War, Edwin Bickford Hooper and his associates have undertaken to appraise the navy's role in world affairs and its changing situation in the increasingly complex defense establishment, as well as its gradual involvement in Vietnam to 1959. They also provide extended coverage of French operations in Vietnam, which often anticipated American naval actions in later years. Later volumes will undoubtedly focus more directly on the navy in Vietnam.

The gradual involvement of the United States in Vietnam is portrayed as a step-by-step process by which the Americans were reluctantly drawn by events to make one commitment after another. The navy historians produce scant evidence to show that the navy influenced the fateful American decisions to accept the return of the French to Vietnam in 1945, to include the French in Indochina in the American military assistance program after the Chinese Communist victory on the mainland in 1949, and to replace the French as instructors and suppliers to the South Vietnamese after Dien Bien Phu. They do point with pride, however, to American naval contributions toward the implementation of established government policies: the provision by the United States of some 550 naval craft to the French in Indochina between 1950 and 1953, the evacuation by the navy of thousands of refugees from North Vietnam after the Geneva accord of 1954, and unnumbered naval demonstrations around the world designed to deter Communist aggression.

The authors also judiciously deplore the steady growth of bureaucracy in the defense establishment that progressively restricted the navy's ability to respond quickly and flexibly to its own and the nation's needs. Not least of the naval casualties in this trend toward "more management and more layers of review, coordination, and direction" was the navy's distinguished General Board, which provided important advice on naval policy to the top echelons in government during the half-century before its abolition in 1951.

Research for this volume was based on the operational records held by the Naval History Division and on the standard published sources. Although their work is likely to stand as the most authoritative survey history of the navy since 1945, the navy's official historians will surely agree that the navy's role in Vietnam will only be fully appreciated as scholars press ahead with truly multi-

archival research in records that are often still unopened.

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CARL M. BRAUER. *John F. Kennedy and the Second Reconstruction*. (Contemporary American History Series.) New York: Columbia University Press. 1977. Pp. xi, 396. \$14.95.

Carl M. Brauer has written a close analysis of President Kennedy's confrontation with the black revolt. He neither deplores the politics of pragmatism nor swoons over Camelot; and while he does not expect the president to lead the troops as a sectarian crusader for civil rights, he applauds its ultimate promotion of the Second Reconstruction. Brauer's major contribution, however, is his ability to marshal the operative factors of the period, the political, economic, and sociological climate, and to place them within the context of the restrictions of a party system and federal government with inherent resistance to centralized authority. More than most students of the period, he appreciates the limitations of presidential power.

Based largely on the Kennedy Library's manuscript and oral history holdings, as well as the author's own interviews, the book rejects arguments holding that Kennedy could have been far more vigorous in behalf of civil rights. Brauer views the administration, and especially the president's personal leadership, as having been crucial in firing the zeal of the activists and imbuing them with "a confidence and daring that they would not otherwise have had." While Kennedy can hardly be given credit for having created the movement, his imprimatur was obvious. Appreciating the political realities of the early 1960s, Brauer contemplates the consequences of the most likely alternative to the Kennedy government. "What would have happened had Richard Nixon been elected President?" he asks. "Would he have sent marshals to Montgomery, would a Voter Education Project have been created, would the Justice Department have dramatically stepped up enforcement under the guidance of someone like Robert Kennedy, would thousands of blacks have demonstrated in Birmingham, and, more important, if they had, would Nixon have responded by proposing and working for enactment of sweeping civil rights legislation?"

Brauer supports his case by assembling pertinent data and clarifying the circumstances. Thus, when coping with Kennedy's segregationist court appointees in the South, he neither condemns nor defends but explains the context. Simi-

lar attention is given to the delayed order desegregating federally funded housing. In addition to the pressures from the South, Brauer points out, several important northern Democrats feared that its untimely issue would jeopardize their own re-election prospects.

Brauer approves of the administration's concern with tempering the explosive potential of both extremes. A Kennedy crusade in 1962 could well have exacerbated an already volatile situation. In both the Albany, Georgia and Oxford, Mississippi crises, the emphasis gave priority to preserving order rather than securing justice, largely through cognizance of legal limitations together with political realities. Describing Martin Luther King's 1963 Birmingham campaign, the author recalls that "no federal statute required lunch counters to serve blacks, department stores to hire them, or local officials to talk to them. Even the most egregious actions taken by Bull Connor . . . did not violate federal law." Finally, the Connors and George Wallaces made their contribution to equality under law by giving the administration the right climate in which to introduce comprehensive legislation. Brauer is confident that the Civil Rights Act of 1964 would have passed when it did even if there had been no assassination.

The book is less convincing when attempting to associate Kennedy's more militant position with his new-found enlightenment about the first Reconstruction. Others will be less charitable about the president's personal devotion to the cause. Brauer is far more perceptive when noting that Kennedy's "personality and view of the Presidency called for decisive leadership and a measure of control over events." Overall, this is clearly the most significant study of Kennedy and civil rights we are likely to have for a long time.

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CANADA

HUBERT CHARBONNEAU. *Vie et mort de nos ancêtres: Étude démographique*. Montreal: Les Presses de l'Université de Montréal. 1975. Pp. 267. \$7.25.

Within little more than a decade's time, the tools as well as the subject matter of social history have been transformed. A prime illustration of these transformations may be seen in the history of the family: it has changed from a collection of random anecdotes and household miscellanea to a consciously broad-based, statistically tested, theoretically informed discipline. Following the lead of

Louis Henry and other French demographers, we now reconstitute the births, marriages, and deaths of our preindustrial ancestors in their parochial or village settings. We have for the first time detailed knowledge of the "life cycles" of ordinary seventeenth-century French and English folk, as well as their colonial cousins who settled in New England, the Chesapeake region, and Barbados. French Canada now joins this list with Hubert Charbonneau's meticulous, refined, technical, and valuable demographic study.

Charbonneau's research on seventeenth-century Canada reveals a society with a mean marriage age of 22 for females and 27.6 years for males, a "low" prenuptial conception rate of 4.5%, a "low" mortality schedule of 20% for children until their tenth year, a common two-year interval between births, and a mean of 7.7 children per couple. This society called 3.5% of its adults to the religious life, its matriarchs approached the biblical promise of three score and ten years, and, like the Israelites of old, they saw their children and their children's children. In short, we have a collective profile remarkably similar—and in many respects richer in texture—to our findings on New England for the same period of time. In both settlements the standard late age for male marriage remained but the female ages were substantially lower than in the European homelands. The result was a higher fertility, a lower mortality for reasons not totally clear, and greater longevity. And both societies kept one-sixth of their adults in the nonmarried state. Just as New England had regional differences, so did Canada: two-thirds of its males came from rural parts of France while two-thirds of the females came from urban areas; in fact, the Paris region supplied 36.6% of all women, Normandy 25.4%, and the West 19.0%. We would like to know more about these females, what areas sanctioned a 20% prenuptial conception rate for widows, and whether regional backgrounds influenced fertility. Yet there can be no doubt that Canada followed ideal values of traditional Christian agrarianism in its northern Eden.

Two patterns emerge from these seventeenth-century studies. The first, which we see in New France and New England, follows the religious and familial ideals of the traditional late-medieval world. Village greens, Norman churches, large families, peasantlike farming, and communal controls mark these settlements in ways never quite found in the Old World. But then neither did their procreativity or the abundance of land, fish, and beaver exist in Europe. The second, which we see in Virginia and Barbados, signifies the market-oriented, commercial world of modernity. Its stress is not on family continuity, persistence, communal controls, and religious callings, but simply

on profit. Both patterns occurred because the *novus mundus* became the social laboratory where early colonialists could both cluster and segregate in ways radically new to the European experience.

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ROBIN FISHER. *Contact and Conflict: Indian-European Relations in British Columbia, 1774-1890*. Vancouver: University of British Columbia Press. 1977. Pp. xvi, 250. \$18.00.

Borrowing Ralph Linton's acculturative model of nondirected (i.e., voluntary) and directed (i.e., coercive) culture change, Robin Fisher has produced an intelligent report on Indian/white relations in late eighteenth- and nineteenth-century British Columbia.

Nondirected culture change occurred during the fur trade era, beginning in the 1770s and ending in the 1850s. For the most part the period was one of creative, enriching acculturation for the Indians. Trade with Europeans and Americans brought unprecedented wealth to the tribesmen, wealth that was plowed back into the community through the potlatch and other redistributive mechanisms.

The author thus argues that until the early 1850s Indian political, social, and cultural autonomy remained intact. Contact with the relatively few traders in the region had enabled them to borrow alien cultural elements selectively, at their pace, without coercion.

Directed culture change took over in the late 1850s, shortly after gold was discovered in the Fraser River. What had until then been a trickle of immigration became, overnight, an avalanche. Prospectors turned into squatters and other settlers soon followed. With the settlers came missionaries and the whole affair was orchestrated by colonial administrators. All three groups set about at once to impose their will on the resident tribes. Settlers clamored for and got Indian land, generally by nefarious means and generally through the cooperation of government officials (with a few notable exceptions). Fisher is superb in his description of dispossession.

The missionaries used a different approach in their attack on the Indian way of life. Unlike the settlers, who tried to bar the Indian from participation in the human community, the missionary labored to incorporate the native into Western society—as a European-Canadian, rather than an Indian. Where the settler reviled the Indian as a "savage" the missionary defended him as a potential white man—and the Indian-as-Indian eluded all of them. In this, too, Fisher is superb.

He is less successful in the first section of the book, in dealing with early European perturbation, where it is misleading to say that the Indians "to a large extent controlled . . . their culture" (p. 24). What about the profound cultural (including spiritual and psychological) dislocations which surely followed in the wake of epidemic disease? What good is it to say these people "controlled their culture" when they may well have lost faith in their traditional belief systems and in shamans' ability to cure disease? Likewise, how are we to interpret the cultural consequences of massive wildlife destruction among a people who had heretofore enjoyed an intimate relationship with their game animals? Apparently Fisher considers all of these issues inconsequential; his Indians march steadfastly on, only ruffled by these experiences.

In fairness to the author, he does discuss the disease issue, although in my opinion he grossly underestimates its demographic and cultural consequences. Studies done of North Atlantic and California coastal tribes show there was a massive and swift die-off in the early years of contact, yet, according to Fisher, Northwest Coast people somehow managed to escape a similar fate. He considers local, contemporary records of Indian decimation too inconclusive to warrant sustained investigation, and so drops the issue. One gets the impression that the author's fondness for his acculturative model has prevented an honest appraisal of the evidence before him.

In sum, Fisher does not seem to have read sufficiently widely in contemporary sources, ethnographic sources, and comparative material, with the result that he has written a distorted section on the fur trade. The internal dynamics of early culture contact have been only partially explored, which is unfortunate, since this detracts from an otherwise first-rate book.

CALVIN MARTIN
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J. L. GRANATSTEIN and J. M. HITSMAN. *Broken Promises: A History of Conscription in Canada*. Toronto: Oxford University Press. 1977. Pp. 281. \$6.95.

For Canadians, "conscription" means much more than compulsory military service. It means compulsory service overseas; it also means a political issue that divided French and English Canadians during two world wars, and threatened to split political parties and destroy federal governments. A study of conscription in Canada touches on a central aspect of national identity.

This volume is an admirable history of the poli-

tics of conscription. It is based on a wide range of primary sources, private papers, and government records, British as well as Canadian. It traces, in a deceptively simple narrative, the pressures on political leaders which in 1914 and again in 1939 produced the initial promises that there would be no conscription, and then traces the developments which led to the breaking of these promises. There are no startling revelations. The attitude of the militant conscriptionists in English Canada is still seen as linked to atavistic bonds with Great Britain, and intensified by the growing animosity against the "disloyal" French Canadians who did not share these sentiments.

Conscription, then, became a way of forcing French Canada to do its part and, for French Canadians, a deliberate effort to force them to conform to the majority will. In the First World War, the politicians are saddled with a good deal of the blame, yielding as they did to the temptation to exploit these emotions for partisan advantage. In the Second World War, some of the politicians had learned something from experience. There were still political leaders who wanted a total commitment to fight at Britain's side even at the risk of cultural divisions at home, but this time the military establishment is also held partly responsible because it deceived the government about the extent of its manpower commitments. Conscription for overseas service was postponed, but in 1944 it was introduced.

Readers may quibble over the details. Arthur Meighen may not have been as politically inept or Mackenzie King as farseeing as the authors believe. No other studies of conscription, however, can match the thoroughness of the research, the readability of the narrative, and the ability of the authors to summarize complex political maneuverings in so little space.

The book does not attempt to deal with all of the issues raised by the conscription crises. The brief introductory chapter does not really explain why French Canadians were almost paranoiac about conscription long before 1917. Why, for example, would Mercier in 1888 and Bourassa in 1910 campaign against English-Canadian imperialism by claiming that Canadian troops in Egypt or a Canadian navy would inevitably lead to conscription? The chapter on the interwar years also passes lightly over the antiwar sentiment which colored attitudes toward compulsory service. Nor do the authors attempt to study the motives of the men who refused to obey the law or of the "Zombies" who refused to volunteer for overseas service.

The authors are not unaware of these aspects of conscription. They have chosen to write a history that is primarily political rather than social or intellectual. They have succeeded so well that

readers will regret that they were not more ambitious.

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PHILIP G. WIGLEY. *Canada and the Transition to Commonwealth: British-Canadian Relations, 1917-1926*. (Cambridge Commonwealth Series.) New York: Cambridge University Press. 1977. Pp. x, 294. \$26.50.

The seventh volume in the Cambridge Commonwealth Series focuses upon Canadian-British interaction from 1900-26 over the issue of consultation in foreign affairs. It illustrates in detail the importance of the issue to the complex constitutional shift from empire to commonwealth. The senior dominion figures prominently because its representatives exercised a major influence in the debates on the main topics considered—the imperial conferences, the peace settlement, the Anglo-Japanese alliance, the Chanak crisis, and the Lausanne and Locarno treaties.

Canadian prime ministers from Laurier to Mackenzie King differed in their perceptions of the British connection and in their methods of dealing with it. Yet all shared a desire to keep Canada free from awkward imperial obligations. As Wigley argues, it therefore would be a mistake to accept Mackenzie King's conviction that some of his predecessors had committed Canada to a centralist imperial policy.

The study's emphasis on Canada and its ministers does not exclude insightful reference to the relations of other imperial participants with Britain and Canada. For example, by demonstrating the contributions of the South African and Canadian prime ministers, Wigley shows that the representatives of the Irish Free State did not single-handedly define equality of status.

Wigley, in effect, synthesizes current research in imperial historiography. By drawing upon recent scholarship, records of British departments, and papers of British and Canadian ministers, he ably demonstrates that the transition from empire to commonwealth followed anything but a linear progression, and that the Foreign Office's concern either to avoid dominion interference or to shed some diplomatic responsibilities contributed significantly to alterations in imperial relations.

Wigley's expositions of the confrontations between the Foreign Office and the Colonial Office over the former's decentralist policies understandably occupy a substantial portion of his study. These cogent discussions vividly illuminate the lack of communication that characterized relations between the two departments. This feature

guaranteed a transition marked by benign confusion and by such classic instances of bureaucratic obscurantism as the episode of the Halibut treaty.

One can criticize Wigley's work on two points. First, an index of names affords limited assistance. A reader searching quickly for a reference to New Zealand might be disconcerted to find that the only reference under N is to Neatby, H. B. Second, Wigley's statement, "By all accounts the 1917 imperial meetings had been a major success," (p. 50) is misleading. The Colonial Office, for example, was annoyed by the lack of results, especially with respect to postwar economic planning. But these are minor flaws, and it should be noted that Wigley tries to integrate imperial economic issues into his treatment. Overall, Wigley's lucid presentation of a complicated issue is a valuable contribution to the history of the British Empire and Commonwealth.

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LATIN AMERICA

HAROLD EUGENE DAVIS *et al.* *Latin American Diplomatic History: An Introduction*. Baton Rouge: Louisiana State University Press. 1977. Pp. viii, 301. Cloth \$15.00, paper \$5.95.

This volume is written as a textbook, designed to furnish "an introductory and interpretative guide for the serious reader and student" (p. vii). As such it is based on secondary literature, particularly general works about Latin America, and seeks to minimize footnotes. Its size is deliberately modest for so broad a task as providing "a comprehensive view of Latin American international history" including "a general Latin American viewpoint" and "the national viewpoints of the several nations" (p. vii).

The study is at its best in providing an overview of Latin American diplomatic concerns and the involvement of the area in world affairs. It focuses on the general problems common to the entire region, the gradual emergence of Latin America on the broader global scene, and its changing role from that of an object of international affairs to that of a participant. The overwhelming role of the United States in Latin American international affairs and the Latin American shifts in different eras from a global concern to a hemispheric focus are effectively presented. The various military conflicts among the Latin American republics are examined in considerable detail, thereby contributing to a clarification of the balance-of-power

factors involved in regional diplomacy. The Latin American nations' relationship to Europe and the United States is, however, discussed far more effectively than their ties to the rest of the world, or to each other.

Inevitably, limitations of size and focus cause difficulties in such a small volume covering so large a subject. The three authors employ divergent approaches, some focusing more on individual states, others on the general factors, some using more detail, and others preferring an overview. Space allocation among the three eras is particularly important and problematic. The initial section, covering the era up to 1860, is 106 pages long, that from 1860 to 1930, 85 pages, and that covering the post-1930 era a scant 75 pages. Hence the space allocated to the era decreases as the complexity of the international situation and the participation of the Latin American nations on the global scene increase. The post-World War II era receives a scant 23 pages. In addition, the focus is clearly on South America, to the detriment of Central America and the Caribbean, though in part this reflects attention to the larger nations. Even in one chapter which contends that the region can be divided into three subsectors, namely Mexico, Central America and the Caribbean, and South America, more than half the space is devoted to the latter area. Throughout the volume coverage of Brazil and Argentina is the most extensive. In most sections, the general Latin American viewpoint is presented far more effectively than those of the individual nations, although despite this limitation the effort to combine the two is commendable, since all too often the views of the individual states are overlooked in North American literature. In spite of these factors, the actions of the United States and the Yankee viewpoint are overly emphasized.

Despite its limitations, the volume will prove useful in its stated purpose of providing an introduction to this complicated and often ignored question. As such, it fills a gap in existing textbooks, and will certainly prove useful to those teaching introductory courses in the international relations and diplomacy of Latin America.

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JULIO HEISE GONZÁLEZ. *Historia de Chile: El período parlamentario, 1861-1925*. Volume 1, *Fundamentos histórico-culturales del parlamentarismo chileno*. Santiago: Andrés Bello. 1974. Pp. 501.

Julio Heise González' book was not written for a North American audience nor entirely for a Chilean one. His work does seek an international pub-

lic, but in this case it is a select group of European intellectuals accustomed to view South American affairs as a response to Western Europe, particularly England and France. A vocabulary abundant with terms like aristocracy, bourgeoisie, and clericalism, references to Hobbes, Marx, and Sombart, and concepts such as historical inevitability and generational conflict all offer guideposts for the Continental mind. Except for unknown names and places, European scholars should find sociopolitical trends so familiar as to wonder whether Chile is located between France and Italy instead of west of Argentina and south of Peru.

This Europeanized vision of Chile is not a figment of Heise's imagination. A former history professor at the University of Chile Law School and author of many perceptive studies of his country's past, Heise remains in the mainstream of the Chilean intellectual tradition. Not only scholars but politicians, labor leaders, businessmen, and artists have all looked to the Continent for guidance. Not by chance, therefore, have local issues reflected trends in Europe. Though this external orientation made Chile more cosmopolitan, one wonders from recent events to what extent all groups overlooked national realities. A major crisis of Chileans today is having to face a country which has swerved away from Western European and more toward Latin American political models.

Though Heise's methodology and perspective are very European, the study does have a definite reference point in Chilean historiography. As a standard-bearer of liberal parliamentary democracy, the author endorses economic and social reform, but always within a constitutional framework. He strongly disapproves of reformers who tried to strengthen the executive at congress' expense. When discussing the Revolution of 1891, for example, he condemns President Balmaceda for causing the conflict by refusing to respect congressional prerogative. For similar reasons the author dislikes Arturo Alessandri's Constitution of 1925. Here Heise sides with critics who claim Alessandri created a presidential dictatorship (p. 455) when he forced his new political organization on congress. For Heise, Alessandri's other reforms failed to compensate for this dangerous shift in power.

Owing to its antiauthoritarian thrust, the publishing of this book in 1974 represents an act of courage. The Chilean Left, however, is not likely to applaud the work any more than the current government; Heise's constitutionalism appeals no more to revolutionaries than to reactionaries. In spite of his personal preference for a powerful congress and a subordinate executive, the author does recognize that new socioeconomic conditions in the twentieth century have sealed the fate of parliamentary democracy in Chile. Whether it was

the author's intention or not, his description of the trend of authoritarianism elucidates not only Chile's past, but its present as well.

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ARTURO VALENZUELA. *Political Brokers in Chile: Local Government in a Centralized Polity*. Durham, N.C.: Duke University Press. 1977. Pp. xiii, 272. \$13.75.

What role did local government play in the unique politics of pre-1973 Chile? Arturo Valenzuela's study, based on field research done in 1969 and 1974, is the first attempt at a systematic answer to this question.

The uniqueness of Chile's politics, prior to the 1973 coup, lay in the degree to which "advanced" constitutional practices became accepted within a framework of economic underdevelopment. To see how this juxtaposition was possible is to understand why it was Chile, rather than an industrialized country, that provided the first practical test of "revolution through the ballot box."

Whichever dimension of Chilean politics one chooses to analyze, the key theoretical issue is to explain the coexistence of certain seemingly incongruous practices, often directly reflecting the division between "advanced" and "underdeveloped" traits. Valenzuela's study highlights several such problematic relationships including 1.) active local political life alongside negligible local economic authority; 2.) minor local-political role of large landowners even where they dominate the local economy; 3.) relatively important role of the mayor despite an unusually rapid turnover in the position; 4.) pre-eminence of nonpartisan service-oriented functions along with strong partisan divisions; and 5.) prime voter concern with "the candidate" together with effective consistency in patterns of party support.

The overall paradox embodied in these combinations is brought out in part one, largely on the

basis of nationwide data which show a.) the thorough penetration of party struggle to the local level and b.) the steady relative decline of municipal budgets. How the high competition and the low stakes fit together is the theme of part two, where Valenzuela presents his 1969 study of fourteen small-town governments in the Bío-Bío region. Drawing on interviews, he describes both the political concerns and the routine activities of local elected officials, whether with their constituents or with their contacts in the capital. His conclusion is that the "ideological" character of national politics and the "particularistic" character of local politics reflected a difference not of culture but rather of circumstance—not a "dual society," but two interrelated aspects of a single system.

Following up on this insight, Valenzuela attempts in part three to show how the role of local institutions has interacted with that of the central government through most of Chile's national history. Although the discussion is not always well focused, it includes some important observations on the fluctuations of local autonomy, seen in terms of such variables as parliamentary vs. presidential system and the magnitude of state revenues from extractive exports.

Valenzuela's 1974 return visit revealed primarily the total break-up of the local infrastructure by the junta. Regarding the Allende years, he indicates only that local politics were overshadowed by national issues. Despite interview opportunities, he does not seem to have probed into how popular participation and local services were affected by the tensions and the innovations of that period. This gap may perhaps be traced to a view of political participation which stresses individual interaction with officials over organized pressure on the policy-making process (p. 89n.). Apart from this limitation, however, the study is both informative and stimulating.

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Festschriften and Miscellanies

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other *Festschriften* and similar volumes that are amenable to reviewing will be found in the review section.

JÜRGEN KOCKA, editor. *Theorien in der Praxis des Historikers: Forschungsbeispiele und ihre Diskussion*. (Geschichte und Gesellschaft, Sonderheft 3.) Göttingen: Vandenhoeck and Ruprecht. 1977. Pp. 224. DM 35.

JÜRGEN KOCKA, Einleitende Fragestellungen. MICHAEL MITTERAUER, Probleme der Stratifikation in mittelalterlichen Gesellschaftssystemen. WINFRIED SCHULZE, Theoretische Probleme bei der Untersuchung vorrevolutionärer Gesellschaften. HORST MATZERATH and HEINRICH VOLKMANN, Modernisierungstheorie und Nationalsozialismus. PETER HÜTTENBERGER, Vorüberlegungen zum "Widerstandsbegriff." KNUT BORCHARDT, Der "Property Rights-Ansatz" in der wirtschaftsgeschichte—Zeichen für eine systematische Neuorientierung des Faches? JÜRGEN KOCKA, Gegenstandsbezogene Theorien in der Geschichtswissenschaft: Schwierigkeiten und Ergebnisse der Diskussion. REINHARD SPREE, Zur Theoriebedürftigkeit quantitativer Wirtschaftsgeschichte (am Beispiel der historischen Konjunkturforschung und ihrer Validitätsprobleme). HELMUT BERDING, Selbstreflexion und Theoriengebrauch in der Geschichtswissenschaft.

RUSSELL MCCORMMACH and LEWIS PYENSON, editors. *Historical Studies in the Physical Sciences*. Number 8. Baltimore: Johns Hopkins University Press. 1977. Pp. xi, 300. \$17.50.

ROBERT H. KARGON, Temple to Science: Cooperative Research and the Birth of the California Institute of Technology. EUGENE FRANKEL, J. B. Biot and the Mathematization of Experimental Physics in Napoleonic France. ROBERT MARC FRIEDMAN, The Creation of a New Science: Joseph Fourier's Analytical Theory of Heat. JED Z. BUCHWALD, William Thomson and the Mathematization of Faraday's Electrostatics. EDWARD MACKINNON, Heisenberg, Models, and the Rise of Matrix Mechanics. DANIEL SERWER, *Unmechanischer Zwang*: Pauli, Heisenberg, and

the Rejection of the Mechanical Atom, 1923–1925. ROBERT E. KOHLER, JR. Rudolf Schoenheimer, Isotopic Tracers, and Biochemistry in the 1930's.

REINHARD RÜRUP, editor. *Historische Sozialwissenschaft: Beiträge zur Einführung in die Forschungspraxis*. Göttingen: Vandenhoeck and Ruprecht. 1977. Pp. 161. DM 13.80.

ARTHUR E. IMHOF, Bevölkerungsgeschichte und Historische Demographie. KARIN HAUSEN, Historische Familienforschung. PETER LUNDGREEN, Historische Bildungsforschung. WOLF LEPENIES, Probleme einer Historischen Anthropologie.

CHRISTIAN GRAS and GEORGES LIVET, editors. *Régions et régionalisme en France du XVIII^e siècle à nos jours*. (Publications de la Société Savante d'Alsace et des Régions de l'Est, number 13.) Paris: Presses Universitaires de France. 1977. Pp. 594.

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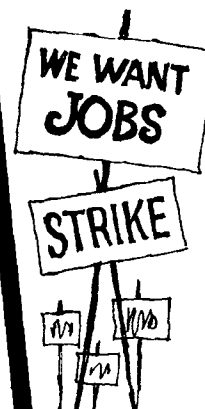
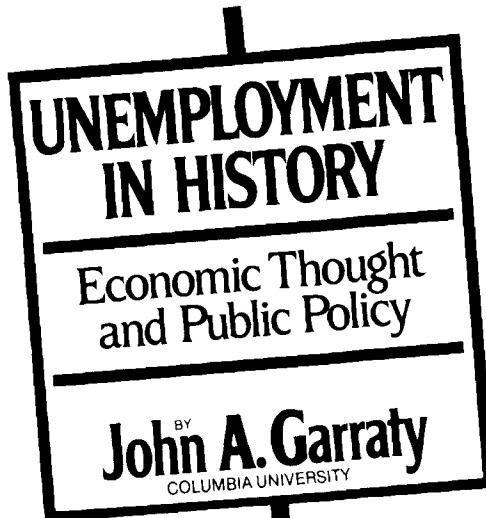
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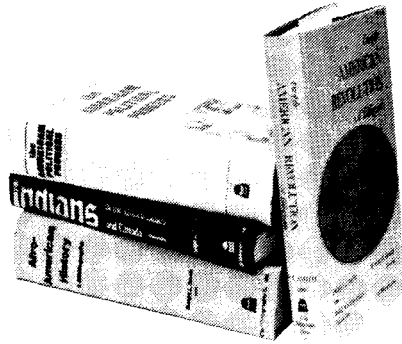
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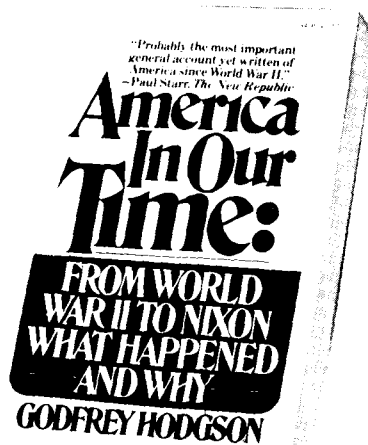
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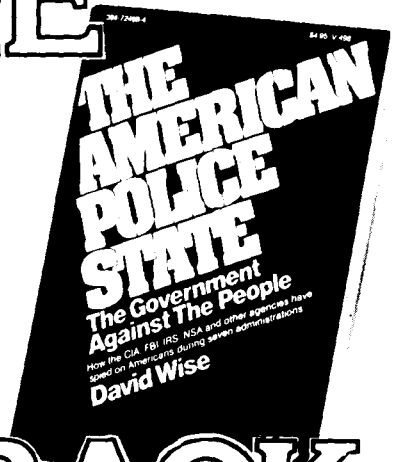


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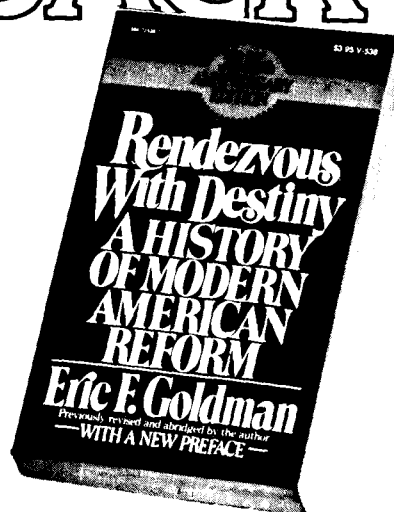


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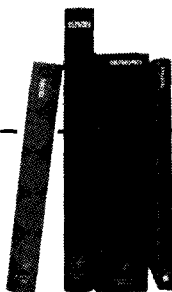
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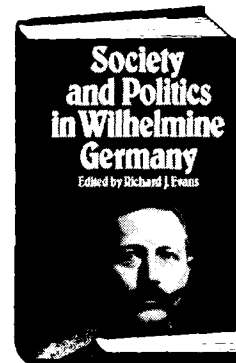
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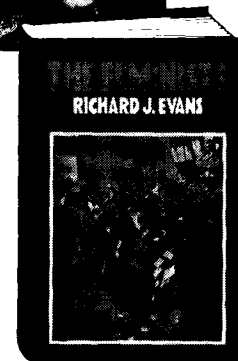
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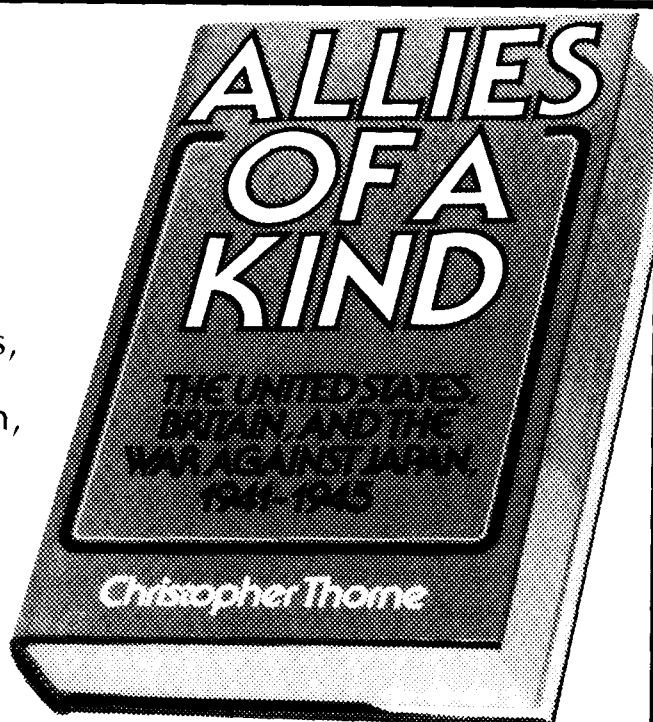
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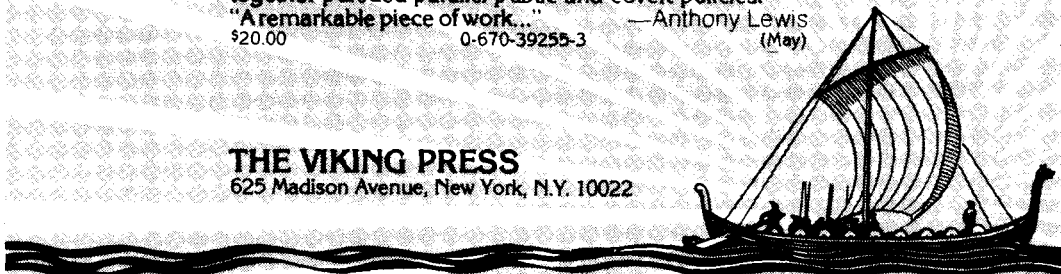
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